This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

+ *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.

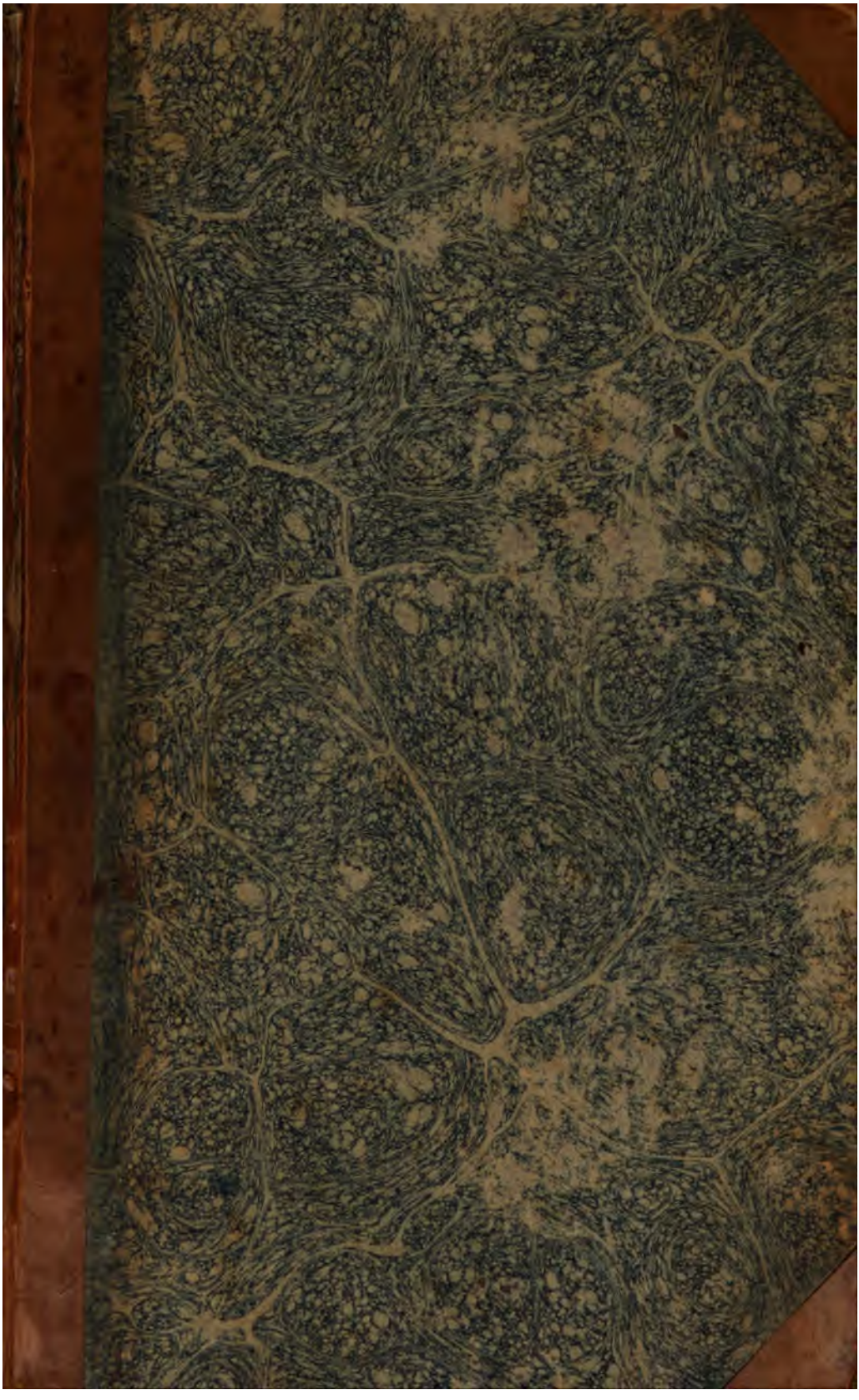
+ *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.

+ *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.

+ *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at `http://books.google.com/`



600044704P

G.12.G.5.

E.BIBL.RADCL.

C
1986 e. 176.

ESSAYS,

CHIEFLY ON

CHEMICAL SUBJECTS.

BY THE LATE

WILLIAM IRVINE, M. D. F. R. S. Ed.

Lecturer in Materia Medica and Chemistry in the University of Glasgow;

AND BY HIS SON,

WILLIAM IRVINE, M. D.

LONDON:

PRINTED FOR J. MAWMAN, 22, IN THE POULTRY,

By W. Flint, Old Bailey.

1805.

TO

MATTHEW BAILLIE, M. D. F. R. S.

THE FOLLOWING

ESSAYS ARE INSCRIBED,

AS

A TESTIMONY OF THE VERY HIGH RESPECT WHICH I ENTERTAIN FOR HIS TALENTS,

AND

THE DEEP SENSE WHICH I SHALL ALWAYS FEEL OF THE FAVOURS WHICH HE HAS SO FREQUENTLY CONFERRED

UPON HIS MOST OBEDIENT SERVANT,

WILLIAM IRVINE.

PREFACE.

EIGHTEEN years have now elapſed ſince the death of the author of the greater part of the following Eſſays, a period of time which has produced or completed one of the moſt ſignal revolutions ever experienced by the ſcience of chemiſtry. That any portion of the writings of Dr. Irvine, ſhould, under ſuch circumſtances, and after ſo protracted an interval, be now preſented to the public inſpection may be ſuppoſed to require ſome explanation. It perhaps cannot well be denied that whatever it is now right to publiſh, would have been better ſo done at a more early period. But this is only true with the admiſſion that ſuch a proceeding could have been adopted with equal or greater convenience than at any ſubſequent

opportunity. The manuſcripts of Dr. Irvine, however, were not by any means in ſuch a ſituation as to admit of immediate publication. A few eſſays, it is true, might have been committed to the preſs without much correction. But in a work of this kind the public has a right to expect, and will unqueſtionably demand a more ample account of the experiments and theory of Dr. Irvine than has hitherto been afforded. Such an account, however, could by no means have been compiled from any manuſcripts in my poſſeſſion. Theſe, every where incomplete, would have left large intervals, the filling up of which would have exerciſed the patience, and have tried the knowledge of the editor. For ſuch an office, not only a conſiderable acquaintance with the ſcience of heat would have been required, but an adequate portion of enthuſiaſm and of intereſt to have ſmoothed the difficulties of the way. The buſineſs of an editor includes much labour, much reſponſibility, and little reward either in fame or emolument. If negligences appear, they are imputed to his want of care; if obſcurities, interlineations, and perplexing diſarrangement obſtruct his way, it is his to find or to make a road through the

wilderneſs; of merit alone he obtains no ſhare, or the ſcanty approbation of unwearied toil.

In general, no doubt, poſthumous works ſhould not be longer delayed than the indiſpenſable attention to correctneſs and propriety demands. But I believe in the preſent caſe it will ſcarcely be denied that ſome further delay was neceſſary. For any complete account of the philoſophical ſpeculations and experiments of my father, he who might have undertaken the publication would have looked in vain amongſt his manuſcripts. The hiatus muſt therefore have been ſupplied by his own exertions, imperfectly aſſiſted by a few ſhort and indiſtinct references to the ſubject of his reſearches. When the difficulties are ſo conſiderable, perhaps unuſual motives are neceſſary to ſtimulate the induſtry of the editor. Some tincture of enthuſiaſm, combined with a competent knowledge of the ſubject, might perhaps be accounted ſufficient for the taſk. The firſt of theſe qualifications may naturally be expected in a ſon who treats of his father's labours. Whether I do or do not poſſeſs the laſt it is not for me to judge.

It has not been, however, without much

anxiety and ſolicitude that I have ventured to uſher into public view any portion of the writings of my late father. In undertaking this duty it has been my object to explain and defend his diſcoveries and theories, as far at leaſt as is conſiſtent with candour and ſound philoſophy. Many of theſe have already found their way to light, and I believe I am not miſled by that partiality ſo natural to my ſituation, when I add that they have conferred a conſiderable reputation upon their author. He has never, however, but from his profeſſorial chair, had the advantage of ſtating his own opinions. This was undoubtedly a voluntary proceeding on his part. But he truſted for the promulgation of his doctrines to teaching them; a mode which ample experience has ſhewn to be at once injurious to the claims of the inventor and to the progreſs of ſcience.

It appears to me, ſince the following Eſſays were not edited immediately after the death of Dr. Irvine, that the preſent is a favourable moment, after the publication of Dr. Black's Lectures, whoſe experiments and doctrines are ſo nearly connected with ſome of thoſe contained in this work, and who was the

immediate precurſor of my father in the inveſtigation of the nature of heat. Every one of the inventors of the modern theories of heat, excepting only Dr. Irvine, has obtained, either from his own labours or from thoſe of a poſthumous editor, an opportunity of explaining and defending his own opinions. But independently of this circumſtance, the theory of my father has afforded a ſubject of conſiderable diſcuſſion to the philoſophical chemiſts. Nor has it been expoſed to this ſcrutiny without having encountered many and various objections from different authors. On the other hand, I know of no author who has taken to himſelf the taſk of examining all theſe objections in a fair and candid manner. Perhaps then it will not appear an extraordinary thing that ſuch a work as the following ſhould be undertaken, nor that the execution of it ſhould fall to my ſhare, connected as I am by the ties of nature to the author of the theory of which it chiefly treats.

In purſuing ſuch a plan, I have believed it neceſſary to deviate conſiderably from my original deſign. I had, at firſt, propoſed to have ſelected, from the manuſcripts of Dr. Irvine, whatever was in a complete ſtate regarding the ſubject of heat. The illuſtrations, the defences,

and the additions, were intended to have occupied the place of notes, which might be referred to at the end of the volume. One argument againſt this proceeding aroſe from the inconvenience of a frequent reference to long annotations, by which the train of ideas is greatly interrupted, and the ſubject after all is inadequately explained. But another and more powerful objection originated in the ſtate of my father's manuſcripts, none of which, excepting only a very few eſſays, were at all complete, or in any reſpect fit for publication. That part of his chemical lectures which treated upon heat, where it was reaſonable to have looked for the fulleſt information, was totally barren, and conſiſted only of heads and references, in which his own peculiar opinions were not at all detailed, and even hardly mentioned. In theſe circumſtances I adopted the plan of prefixing a few eſſays of my own compoſition, in which, to the beſt of my power, I ſhould introduce the ſubject, endeavour to explain the doctrine of capacities, and the important inferences to which it has led, and to ſubjoin an anſwer to ſuch objections, from theory or experiment, as I know to have been hitherto made to it. It may, at firſt ſight, appear to

ſome ſtrange and even indecorous, that I ſhould have given the firſt place to the eſſays of my own writing: but I hope this will not be attributed to any improper feelings upon my part, but to the nature of the ſubject, which required that the topics explained in my eſſays ſhould precede the reſt.

I have, according to the views now mentioned, divided the following work into three parts. In the firſt of theſe, I have endeavoured to give an ample and accurate account of Dr. Irvine's ſpeculations upon heat: in the ſecond, I have placed ſuch of his writings as appeared to me to admit of publication: in the third of theſe diviſions, I have ventured to add a very few eſſays, for the matter and compoſition of which I am myſelf ſolely reſponſible.

The firſt part contains four eſſays. In theſe I have detailed every fact and every argument with which I am acquainted, either favouring or oppoſing the doctrine of capacities. If I have occaſionally digreſſed a little from the main object of theſe Eſſays, and hazarded ſome obſervations upon parts of the theory of heat, which are not immediately connected with the ſubject of our inquiries, I hope for my excuſe in the candour of the ſcientific reader, who

cannot but be aware how alluring the regions of imagination appear amidſt the more ſolid but leſs attractive details of facts. In ſome inſtances I have made, what appeared to me to be needful additions; and it has been my aim, in all places, to defend the theory from thoſe errors and inconſiſtencies with which it has been charged, and which, it will be readily obſerved, have, in my opinion, never exiſted, but in the minds of thoſe who have taken a falſe or partial view of the ſubject.

It may, perhaps, be looked upon by many as a taſk of no conſiderable difficulty to collate the doctrines of a philoſopher, to whoſe writings I have had the moſt unreſtrained acceſs; and this would have been perfectly juſt, provided theſe writings had been fully expreſſed, or had not been almoſt always totally ſilent on the ſubjects of which I have treated. Had any ſuch diſtinct ſtatement appeared amongſt the manuſcripts of my father, I ſhould have been far from preſuming to replace, by compoſitions of my own, any illuſtrations which he might have given of peculiar, dubious, or intricate points. But, unfortunately, there has been no alternative, between paſſing over the ſubject in ſilence, and aſſuming to myſelf the taſk of ex-

plaining it. I am aware that I ought to ſuſpect myſelf of prejudice upon theſe topics; nor am I in any reſpect aſhamed to own my feelings of partiality. But after making every allowance for this, I ſtill think that the theory of heat, propoſed by Dr. Irvine is in its own nature ſo curious, as to deſerve and to require a fuller explanation than it has hitherto received. This is peculiarly true with reference to its elementary points. Beſides which it has been frequently, and not mildly, attacked, yet ſcarcely ever defended for a conſiderable number of years. It is not to be inferred from this that the theory has been abandoned by the chemical philoſophers of the day. To demonſtrate the reverſe of that, I have only to mention the ingenious treatiſes of Profeſſor Leſlie, of Mr. Nicholſon, and of Mr. Murray. But theſe gentlemen, contented with explaining or adopting the theory in a general way, have not entered ſo minutely into details, which indeed would have been inconſiſtent with the plans of their works; nor have they attempted to follow all the objections of preceding and contemporary writers, the inſufficiency of which it has been my endeavour to point out.

There were other parts of the ſcience of heat

beſides thoſe immediately neceſſary to the ſupport of his theory, which had attracted the attention of Dr. Irvine. Wherever I have become acquainted with any obſervations or experiments of his upon theſe ſubjects, I have taken an opportunity of introducing a notice of them in theſe preliminary eſſays.

The ſecond part of the following work is compoſed of eſſays written by Dr. Irvine, and which were many of them read, and all of them intended to be read, before the "Literary Society of Glaſgow," between the years 1769 and 1784. Each eſſay is marked by its peculiar date wherever I have been able to aſcertain that point. Since the period at which they were compoſed, the changes in the ſcience of chemiſtry have been immenſe, and altogether aſtoniſhing. That branch of knowledge has advanced with gigantic ſtrides, and has reared its head to a height which the boldeſt ſpeculator could not have foreſeen or expected. In theſe Eſſays, however, I hope little will be found inconſiſtent with the preſent ſtate of chemiſtry. I have, in general, where alterations have taken place, marked them in the margin; at leaſt, where they appeared to me of ſufficient importance, or at all likely to

miſlead. It ought not to be expected, however, that eſſays brought forward under the preſent circumſtances ſhould contain, or be made to contain the more recent improvements. In ſelecting them from the manuſcripts of Dr. Irvine, I have been guided by different principles. It appeared to me, wherever I could find any complete eſſay regarding heat, in which peculiar doctrines were maintained, that ſuch an eſſay would be acceptable to the public, though they may already be in poſſeſſion of the principal facts contained in it. There is always a deſire to underſtand the terms and method employed by any diſcoverer or inventor of theories.

I have alſo added to the collection all the tracts in which any new principles or facts are announced, of which the public are not in poſſeſſion. In general I have offered to the world whatever I have found amongſt the manuſcripts of Dr. Irvine which appeared to my conception to contain novelty, or to be treated with peculiar ingenuity or propriety. In theſe procedures I have been guided almoſt intirely by my own judgment and taſte. The candour of the public, therefore, will not fail to direct that cenſure which it may think pro-

per to adjudge into its proper channel, if in this reſpect there ſhall appear juſt cauſe for cenſure. It will not be difficult for an accurate obſerver to perceive that none of theſe Eſſays were ever intended for the preſs. It would be unfair, therefore, to attribute to their author any other inaccuracies than thoſe of reaſoning; and even of them, where they exiſt, it cannot be doubted that the hand of the original compoſer could have extinguiſhed or abridged the number. Where I have been able, I have added the corrections which appeared to me needful. But I am reluctant that thoſe blemiſhes which may have eſcaped my diligence or eluded my obſervation ſhould occaſion cenſure to any other than their editor.

Upon the whole, however, the very deſign of publication ſhews, on my part, a conviction of a different reſult, and I am perſuaded that the public will not receive without ſome degree of intereſt theſe ſpecimens of the method of reaſoning and experimenting employed by Dr. Irvine. The purſuits of an ingenious mind cannot but afford amuſement as well as inſtruction, and I am impelled to beſtow ſome confidence on the remark of the late learned Dr. Robinſon, combined as it is with a

moſt unmerited compliment to myſelf, "that the ſtudies of ſuch a man as Dr. Irvine muſt have been extremely ingenious and important." I will now conclude my prefatory remarks on the ſecond part of this work, and only further obſerve that, if I do not deceive myſelf, there will be occaſionally found in theſe Eſſays, remarks and views which even now are not familiar to the cultivators of philoſophy.

There is another eſſay of Dr. Irvine on nervous diſeaſes which has always appeared to me to contain ſome novelty and ingenuity of conception, and to which I ſhould, perhaps, have given a place in this work, but that I have already made it the ground-work of an inaugural diſſertation, "de Epiſpaſticis," which I publiſhed at Edinburgh in the year 1798.

With regard to the third part of the work, I have little to obſerve. As it is by my voluntary deed preſented to the public, it muſt await and ſubmit to their judgment. The eſſay on latent heat was formerly printed in Nicholſon's Journal, though in a more imperfect and abbreviated form.

The ſubſtance called formerly black man-

ganeſe, and now black oxyde of manganeſe, has been long known and employed in the arts. The diſcovery of the metallic nature of this ſubſtance has been hitherto chiefly attributed to the Swediſh chemiſts. What I am now about to advance cannot be regarded as an attempt to deprive theſe philoſophers of any part of their well-earned merit. But ſince I do not entertain any doubt of the ſame diſcovery having been made in Britain at rather an earlier period than in Sweden, it would be unjuſt to omit the mention of it. The difference has been that in the latter country it has been committed to the preſs; while in the former, another mode of communication, that of teaching it, was adopted, not equally ſucceſsful in diſſeminating the knowledge of the facts.

Amongſt the notes of experiments performed by Dr. Irvine, which are in my poſſeſſion, there appear a very great number on the black oxyde of manganeſe. In theſe he formed almoſt every combination of which that body is ſuſceptible, and even produced and remarked ſome of the peculiarities of oxi-muriatic acid gas, though he was unable to condenſe it, having attempted to receive the

product in a receiver without water. Of conſequence the extraordinary properties of that ſubſtance were not detected by him. In referring to the article manganeſe, in the notes which he employed for his chemical lectures, I find the following paſſage, which contains the greater part of his obſervations upon this ſubject.

"In the year 1769 I was engaged in experiments on the colours uſed in painting on enamel. Among other ſubſtances manganeſe was examined; and as in ſome caſes the colour which it communicated to glaſs reſembled that which is given by gold, I hoped I ſhould be able to prepare it in ſuch a manner as to make the colour ſtill more beautiful. I accordingly ſubjected it to many operations, and particularly digeſted it with all the mineral acids, and from the phenomena which I obſerved in mixtures of manganeſe and acids, as well as from the colour, I concluded it was a metallic ſubſtance; but whether a new metal, or a mixture of thoſe already known, I could not at firſt determine. Upon examining the ſolutions in acids more carefully, I concluded that it contained a metal of a peculiar kind, and this is evident from the following effects of acids

upon it. It ought to be obſerved, however, that beſides a new metal, manganeſe often contains ſome old metal. The manganeſe which I employed in all my experiments is that uſed by the potters and at the glaſs-houſes, commonly called by the French manganeſe *de verriers*. It is found at Mendip hills.

"Manganeſe reduced to powder and expoſed to heat retains its black colour, and if the heat is raiſed to bright red, the powder coheres, ſo that it cannot be ſeparated from the veſſels. The alteration is inconſiderable, unleſs the manganeſe was mixed with a large quantity of calcareous ſpar, which is not commonly the caſe. I generally picked out this ſpar as well as poſſible before I expoſed the maſs to heat. In a violent fire it runs into a dark-coloured ſlag, which, however, is never in complete fuſion. It is uſual to calcine the manganeſe before it is uſed in painting, which is perhaps of no great importance.

"Manganeſe, mixed with the different foſſil acids, is more or leſs diſſolved in them. It diſſolves with moſt violence in common ſmoking nitrous acid, more completely in muriatic acid, but ſooneſt and beſt in aqua regia compoſed of two parts of nitrous acid, and one

of muriatic acid, in which it may be completely diſſolved. The whole does not admit of ſolution in nitrous acid; but the part which is undiſſolved having all the properties of manganeſe, I imagined that this was owing to the want of phlogiſton. Accordingly, on heating it red-hot with charcoal duſt, and then adding nitrous acid, it diſſolved completely with violent effervescence. This firſt convinced me that it was a metal of a particular kind. By digeſting manganeſe in ſpirit of wine, it may be all diſſolved in nitrous acid, and this may be done even by expoſing the manganeſe and acid to the ſun's rays. By theſe means manganeſe may be completely diſſolved in all the acids, and by my experiments there never remained $\frac{1}{100}$ part, and in ſome caſes nothing. The beſt way of getting a pure ſolution of manganeſe is, firſt to calcine it, then to infuſe in the nitrous acid, which will take up the calcareous ſpar, &c. mixed with it, then to waſh it well, dry it, and phlogiſticate the remainder in ſome of the ways mentioned above. It may then be diſſolved in the acid.

" Fixed alkali precipitates manganeſe from acids in form of a white powder, which, laid

on a red-hot iron, burns and becomes black. Treated in a phial as in the operation for Homberg's pyrophorus, it forms ſomething like it, but it requires more heat to ſet it on fire."*

Amongſt the other notes of theſe experiments upon manganeſe, I obſerved an alluſion to an experiment of the celebrated Mr. Watt of Birmingham, in which he had been able to procure a few metallic globules from manganeſe by heating it in contact with combuſtible ſubſtances. I was induced by this to write a letter to Mr. Watt, requeſting of him to inform me whether he had made ſuch an experiment,

* In another place I find the following paſſage. "An ounce of manganeſe and two drachms of crude tartar well mixed in fine powder, in a covered luted crucible, were made red hot for fifteen minutes. The upper part was brown, the under green, and the middle purple mixed with dirty green. It had concreted a little, but crumbled between the fingers. When not hotter than boiling water, it ſet fire to paper like Homberg's pyrophorus." This experiment has not given the ſame reſult in my hands. A more expert operator might be more ſucceſsful. Perhaps this combination, probably one of charcoal and manganeſe, may throw ſome light on the varieties of metallic manganeſe, ſome of which manifeſtly turn black powder on expoſure to air, others do not. It muſt be remarked that all the experiments from which Dr. Irvine inferred the metallic nature of manganeſe, are not related above; indeed only a very ſmall part.

and if he was acquainted with any ſpeculations of Dr. Irvine upon this ſubject. In return to which I was favoured with an anſwer containing the following paſſage, fully confirming, by the teſtimony of a living and moſt highly reſpectable evidence, the truth of what has been above aſſerted.

"I remember perfectly the experiment you mention, about the year 1769. The globules were found principally attached to the cover of the crucible, and were afterwards diſcovered to be lead alloyed with manganeſe, as they ſtained glaſs purple. Seeing theſe metallic globules, ſuggeſted to your father the trying of the manganeſe in the way of ſolution, when he found its ſolutions had the properties of thoſe of a metal. And theſe facts took place long before we heard of the Swediſh experiments on that metal. We, however, were never able to reduce it to a regulus, at leaſt that I know of."

Since the above communication from Mr. Watt, I have learnt from ſome other gentlemen that the experiments of my father were well known at that period, and that he was preſſed by his friends to offer them to the public, which however, he unfortunately neglected to do in

any other way than by detailing them in his lectures. I may mention the name of Dr. Patrick Wilſon, late profeſſor of aſtronomy in the univerſity of Glaſgow, as one of thoſe gentlemen from whom I received this information.

From the ſtatement now given it appears that Dr. Irvine and Mr. Watt have joint claims on the diſcovery of the exiſtence of a peculiar metal in black manganeſe. Their experiments were fully more early than thoſe of the Swediſh philoſophers Bergman, Scheele, and Gahn. The inveſtigation was undertaken in both countries, in all probability, without the ſlighteſt knowledge of ſimilar attempts being made in another place.

I have introduced the mention of theſe facts into the preface, becauſe I did not perceive any opportunity of inſerting them with advantage in the body of the work.

I may notice here, that the freezing of the water in the calorimeter obſerved by Mr. Wedgewood, as remarked in Eſſay IV. Part I. page 130, was alſo found to occur by Mr. Accum, as I am informed by himſelf, in experiments made with the calorimeter in the laboratory of the Royal Inſtitution. This

information I did not receive in ſufficient time for inſertion in its proper place.

It is alſo proper to remark, that M. de Luc has juſtified himſelf from the charges preferred againſt him by Dr. Robinſon, in a letter addreſſed by him to the writers of the Edinburgh Review.

In the firſt eſſay I have made ſome very brief obſervations on the queſtion of the neceſſity of the particles of matter not touching each other, in which I have endeavoured to ſhew that a partial contact might be made to agree with the phenomena. Since writing theſe remarks, I obſerve in Nicholſon's Journal, that a French author has entertained the ſame opinion, with which, however, I was intirely unacquainted at the time of writing my remarks.

Since the firſt part of the following Eſſays was printed, I have met with ſome obſervations of M. Berthollet in his Chemical Statics, on the ſubject of heat. He there diſcuſſes the merit of the theory conſidered in the following work. After noticing the doubtful arguments which have been deduced by M. M. Lavoiſier and Laplace, from the wide variations of the natural zeros, calculated according to their

experiments in the calorimeter, he adds, that they found the capacity of a ſolution of one part of nitre in eight parts of water leſs than ſhould have been indicated by the expreſſion of the ſpecific heat of the water of the ſolution alone, which would have been .88889, whereas by experiment it was only .81670. Cold is produced in the formation of this ſolution, and the capacity, inſtead of being leſs, ought, according to the theory, to have been greater. This argument, if perfectly eſtabliſhed, would certainly, as I have obſerved in the following Eſſays, be deciſive. I need not here, however, repeat the very ſtrong objections which can be propoſed againſt the accuracy of the calorimeter, on which, in this caſe, every thing depends. But I reſolved to repeat the experiments on the capacity of the ſolution in queſtion with as much accuracy as poſſible in the way of mixture.

The medium of compariſon which I uſed was pounded glaſs. The capacity of various glaſſes being very different, I firſt found by experiment the glaſs uſed to have a capacity of .190. I then made three experiments, mixing this glaſs with the ſolution. Each time I uſed five ounces of each of the materials at

different temperatures, and the results approached each other as nearly as is to be expected. The mean deduced capacity was .914. The temperature of the glass was never higher than 120°, nor that of the solution lower than 44°.

In offering my experiments, in opposition to those of men so illustrious as the authors of the Memoir upon Heat, I am not insensible of the risk of the accusation of temerity. Nor is it without feelings of the most unfeigned diffidence, that I venture to oppose the opinions of philosophers of so high a character. My excuse must be "amicus Plato, amicus Socrates, sed magis amica veritas."

In the Essay I. Part I. I have mentioned an argument illustrating the possible decomposition of water in the galvanic trough, and the appearance of the hydrogen and oxygen at a distance from the spot where they must have been produced. I allude to the similar occurrence in the decomposition of the acetite of lead by zinc. It is proper to state that I learned this argument in a conversation with Mr. Nicholson, though I am uncertain whether he claims the invention of it.

CONTENTS.

PART THE FIRST.

ESSAYS ILLUSTRATIVE AND EXPLANATORY OF DR. IRVINE'S THEORY OF HEAT - - - - 1

ESSAY I.

On the Nature of Heat - - - - - 3

ESSAY II.

On some of the principal Discoveries made by Help of the Thermometer - - - - - 41

ESSAY III.

On the Capacities of Bodies for Heat - - - 77

ESSAY IV.

On the lowest Degree of Heat - - - 103

PAGE

PART THE SECOND.

ESSAYS ON VARIOUS SUBJECTS - - - 161

ESSAY I.

On Heat produced by Mixture - - - 163

ESSAY II.

On the Effects of Heat and Cold on Animal Bodies 191

ESSAY III.

On Evaporation - - - - - - 207

ESSAY IV.

On Rain - - - - - - - - 227

ESSAY V.

On the Fertility of Soils - - - - 255

ESSAY VI.

On the Seeds of Plants - - - - 273

ESSAY VII.

On the Roots of Plants - - - - 289

PAGE

ESSAY VIII.

On Fermentation - - - - - - 307

ESSAY IX.

On the same - - - - - - - 327

ESSAY X.

On Cements - - - - - - - 345

ESSAY XI.

On ancient Cement - - - - - 367

ESSAY XII.

On the Diamond - - - - - - 383

ESSAY XIII.

On the Quantity of Matter in Bodies - - 407

ESSAY XIV.

On Water - - - - - - - 427

PAGES.

PART THE THIRD;

CONTAINING EXPERIMENTAL ESSAYS - - 445

ESSAY I.

On Latent Heat - - - - - - 447

ESSAY II.

On the Affections of Sulphur with Caloric - - 473

PART THE FIRST.

ESSAYS

ILLUSTRATIVE AND EXPLANATORY

OF

DR. IRVINE'S THEORY OF HEAT.

BY WILLIAM IRVINE, M. D.

"Veteris vestigia flammæ."

ESSAY I.

ON THE NATURE OF HEAT.

THE idea of heat is perfectly familiar and ſimple. In common language every perſon underſtands by that word a peculiar ſenſation excited in the human body by the approach or contact of hot ſubſtances. In the philoſophical acceptation, the phraſe heat is uſed to expreſs the cauſe of this ſenſation, whatever that may be.

Heat has been the ſubject of many inquiries, and a number of its properties has been ſucceſsfully inveſtigated. From its univerſal diffuſion and its preſence or interference in every chemical proceſs of nature or of art, it has attracted an unuſual portion of the attention of chemical philoſophers. Nor can it be ſaid to be unworthy of the inveſtigation which it has received, ſince in theory it forms one of the

moſt curious purſuits, and in practice one of the moſt uſeful and moſt univerſally applicable to the purpoſes of life of any of the objects of ſcientific reſearch.

In the new nomenclature the word caloric has been adopted to expreſs the cauſe of the ſenſation of heat, and therefore bears exactly the ſame meaning which the word heat has always done in ſcientific works ſince the attention of philoſophers has been directed to this ſubject. There is ſurely now little or no danger of ambiguity from the confuſion of the cauſe with the effect of heat, whatever may have been formerly the caſe. I am therefore inclined to conſider theſe two words as poſſeſſing almoſt equal merits, and to be ſo nearly balanced in the ſcale of excellence, that I have indulged myſelf in the indiſcriminate uſe of both, guided by no other motives than thoſe of convenience. In this procedure I think I am juſtified by the practice of the beſt and late chemical writers of this country. Verbal criticiſm is not often uſeful in ſcience. If either of the words in queſtion be clearly defined, we ſhall be at no loſs to underſtand the ſubject of our arguments by whatever name it may be called. Beſides the philoſophy of heat has received many, I believe I may ſay moſt of its improvements in this iſland; and in the writings of the Britiſh chemiſts the word heat has been always at leaſt partially employed. For theſe reaſons, to ſay nothing of the circumſtance that the propriety of the phraſe caloric itſelf

has been queſtioned, I ſhall uſe in the following pages indiſcriminately both theſe modes of expreſſion.*

Heat or caloric is univerſally diffuſed. No ſubſtance has been obſerved to exiſt totally deprived of it. It may be partially withdrawn; a body may be rendered greatly colder than it ever appears naturally at leaſt in theſe latitudes; but there is not the moſt remote reaſon to imagine that a body in ſuch a ſituation does not ſtill contain a large ſupply of caloric.

Till very lately it was not ſuppoſed that heat ever exiſted ſeparately from one or other of the various ſpecies of matter compoſing this earth, or from the rays of light. Perhaps it may even yet be aſſerted, that no philoſopher has been able indiſputably to produce a quantity of caloric without employing as a vehicle for it ſome ſubſtantial body. Hence it has been difficult to advance poſitive proof that heat is matter.

The curioſity of philoſophers, however, has not been reſtrained by ſuch embarraſſments. Many opi-

* The whole quantity of heat in any body was uſually named by Dr. Irvine its abſolute heat, in contradiſtinction to relative heat; and when the whole caloric contained in any ſubſtance is conſidered, this expreſſion is peculiarly clear and convenient, and has accordingly been adopted by moſt writers on this ſubject. The abſolute heat or caloric of any body therefore means the whole quantity of the cauſe of heat reſiding in that body, in whatever ſtate or combination that may be.

nions have been propoſed concerning the nature of caloric. Some of theſe have regarded it as a compound: others, on the contrary, as a ſimple ſubſtance. Whilſt a number of philoſophers have refuſed altogether to acknowledge its material nature, and are diſpoſed to account for the phenomena of heat by ſuppoſing a certain tremulous or vibratory motion among the ultimate particles of which bodies are compoſed.

The illuſtrious Scheele conceived that caloric was compounded of oxygen and phlogiſton, and that light was a compound of the ſame nature with different proportions of the ingredients. This theory accounted very well for a great number of facts, and was not unworthy of its celebrated author. It is now, however, only mentioned in the hiſtory of opinions once held upon this ſubject, ſince the foundation of the whole ſuperſtructure has been removed by the revolution which has expunged the name of phlogiſton from the liſt of chemical agents.

A much more recent hypotheſis has repreſented caloric as compounded of poſitive and negative electricity. The phenomena of the galvanic trough have given riſe to this opinion originally propoſed as far as I have learned by Dr. Gibbes of Bath. It is a well known fact, that while the elicitation of poſitive and negative electricity is going on from the different ſides of the pile or trough, a ſmall ſtream of oxygen and hydrogen gaſes is diſcharged, the

oxygen gas from the ſide which produces the poſitive electricity, and the hydrogen gas from that which produces the negative. From theſe circumſtances it is inferred that it is the combination of water with the poſitive electricity which forms the oxygenous gas, and the union of water with the negative electricity which forms the hydrogenous gas. When therefore theſe two gaſes are forced to combine, their water muſt be depoſited, and the poſitive and negative electricity unite in the form of caloric. Thus far this hypotheſis gives a very clear and ready account of the facts. Before admitting it, however, it muſt undergo a more ſevere inveſtigation.

One of the circumſtances which has given origin to Dr. Gibbes' theory has been the decompoſition of water in the galvanic trough, the two component or ſuppoſed component parts being given out at a conſiderable diſtance from each other. Richter imagined that this fact ſufficiently proved the falſehood of the theory of the compoſition of water firſt propoſed by Mr. Cavendiſh and Mr. Watt, and commonly called the Lavoiſierian theory. He could not conceive that the ſame particle of water could be influenced at two different places at the ſame time. Now this is certainly a conſiderable difficulty, and it may not be eaſy altogether to remove it in the preſent ſtate of knowledge of the action of the particles of matter on each other. But it can be readily ſhewn that ſimilar embarraſſments oppoſe

the explanation of various other chemical phenomena. If, for example, a piece of zinc be hung in a ſolution of acetite of lead, the lead is ſeparated in the metallic ſtate, and forms a very beautiful tree. At the beginning of this proceſs a particle of zinc meets with one of acetite of lead, which it decompoſes, and the lead is depoſited at the point of action. This happens, however, only at the beginning; for as the decompoſition goes on, the lead is depoſited farther and farther off, and near the end of the operation at a very conſiderable diſtance from the zinc. This is undoubtedly a very curious and difficultly explicable phenomenon, and reſembles the caſe of the decompoſition of water by galvaniſm in the main point, to wit, the depoſition of the parts of the compound at a diſtance from each other. Yet this, as far as I know, has not been hitherto ſuppoſed a ſufficient ground for denying that acetite of lead is formed by the union of acetous acid and oxyde of lead.

Beſides this ſeparation of the parts of a compound is not by any means a very rare occurrence. It may be obſerved in all precipitations of metals in the metallic ſtate. The arbor Dianæ ſhews phenomena extremely ſimilar. If attention be paid to the formation of cryſtals in any ſaturated ſolution, of muriate of ſoda, for example's ſake, it is, I believe, admitted that if evaporation be prevented, no cryſtals will be formed. But if evaporation be permitted to proceed, the ſalt ought to be depoſited at

the ſurface, becauſe each particle of the compound of ſalt and water is there decompoſed. But it is a certain fact that the depoſition very often takes place at the bottom of the veſſel. So that here a decompoſition is effected at a given point, and one of the ingredients makes its firſt ſeparate appearance at a conſiderable diſtance from that point.

Theſe examples occur to me as ſufficient to ſhew that, ſuppoſing water to be compounded of oxygen and hydrogen gaſes, the production of theſe gaſes in the galvanic trough at a diſtance from each other is not an unique phenomenon in chemiſtry. Whence it ſhould follow, that it is not neceſſary to invent a peculiar theory to explain it, or at leaſt that this theory ſhould apply in the other caſes as well as in the preſent. Now, though it may be poſſible, and is not by any means unlikely, that electricity may interfere in many chemical proceſſes where its agency is not now ſuſpected, it does not appear how Dr. Gibbes' hypotheſis can apply to the decompoſition of acetite of lead or ſaturated ſolutions of various ſalts.

Beſides, as has been well obſerved, water of ammonia being expoſed to the galvanic action, hydrogen and nitrogen are produced, and no oxygen at all, which cannot be reconciled with the principles in queſtion. It is alſo extremely doubtful whether the two kinds of electricity can be conſidered as diſtinct ſubſtances, which ought to be aſcertained before ſo important deductions be made from it. Upon the whole, therefore, it appears that though we can ob-

ſerve much ingenuity in this theory of Dr. Gibbes, there are not yet brought forward ſufficiently ſtrong proofs to convince us of its truth.

Of the other hypotheſes of the nature of caloric one conſiders it as a ſubſtance of extreme ſubtilty, penetrating with eaſe even the denſeſt and hardeſt bodies, contained in conſiderable quantities in all matter, and the particles of which repel each other the more ſtrongly the nearer they are forced to approach. This opinion receives ſome degree of illuſtration, if not of ſupport, from the analogy of the electrical fluid. Between caloric and electricity, indeed, there ſeems to be a conſiderable but obſcure analogy. Neither of them has hitherto been proved to be material, by the teſt at leaſt of gravitation. Electricity can be ſubſtituted for heat in giving origin to combuſtion and in fuſion, and reſembles it in the property common to both of being accumulated by friction. This analogy, however, is very far from being complete. Fuſion by electricity is certainly materially different from that by caloric, ſince a metal may be melted by electricity while the cloth in which it was enveloped remains untouched. It probably conſiſts in a momentary ſeparation of particles from the repulſion ſuppoſed to be communicated to them by the electric power. Caloric in many inſtances may be ſuppoſed to fuſe in a ſimilar manner, at leaſt in thoſe caſes where the ſolid is dilated during its paſſage into the fluid ſtate. It has been obſerved, however, by Van Diemen, that metals, after

ſuſion by electricity, are left with a looſe powdery appearance. The cauſe of this may be, firſt, that the electricity not being repelled by particles ſimilar to itſelf, paſſes off with greater rapidity than caloric can do, which meets caloric on every ſide: ſecondly, that the capacity of the fluid for caloric is enlarged, or in other words, the fluid contains much latent heat, which it muſt loſe before becoming ſolid, and therefore a conſiderable ſpace intervening before the ſolidification is finiſhed, the particles have time to arrange themſelves, as directed by their mutual attraction for each other. Probably then the capacity of a body for electricity is not much increaſed by its fuſion by the electric matter, unleſs the want of electric matter in the ſurrounding bodies be thought ſufficient to account for the rapid diſperſion of that for a moment accumulated in the fuſed body.

All ſolids become hotter by being rubbed; a certain claſs only accumulate electricity. Some bodies do not tranſmit the electric fluid at all, or with extreme difficulty; all bodies tranſmit heat, and though there are different conducting powers of different ſubſtances for caloric, yet this difference is by no means to be compared with the reluctance of the non-electrics to tranſmit electricity. It is poſſible that many of theſe inconſiſtencies may diſappear before a more accurate and extenſive knowledge of heat and electricity.

The exiſtence of heat, as a peculiar ſubſtance, is very hypothetical. It muſt therefore fall to the lot

of thoſe who defend that opinion, to ſhew that ſuch a ſuppoſition will account for all the phenomena, and that it is impoſſible to explain them without it. If they fail in either of theſe, their argument cannot be juſt. If caloric be matter, it at leaſt cannot be ſhewn to gravitate. Many experiments have been made to determine this point. At one time theſe were ſuppoſed to prove that caloric really did gravitate. Afterwards it was imagined that more accurate trials had proved that it not only did not gravitate, but actually leſſened the former weight of the body containing it, and was a principle of levity. And very ingenious theories were deviſed to explain this curious phenomenon. It ſeems now agreed, however, from the lateſt and beſt experiments, that caloric poſſeſſes neither weight nor levity, and does not affect the gravity of the body into which it enters.

Caloric paſſes, though very ſlowly, through the Torricellian vacuum. This has been conſidered as a proof of its material nature, ſince a quality or ſpecies of motion of a vibratory or other kind cannot, by any ingenuity of hypotheſis, be ſuppoſed to penetrate vacuity. And perhaps this argument might be concluſive enough, provided it could be ſhewn that the Torricellian vacuum is really a portion of ſpace unoccupied by matter. But it can only be ſhewn to be a vacuum of air, and may, nay certainly does, contain light, electricity, and perhaps other inviſible, and yet unknown ſubſtances.

The ſtrongeſt arguments for the exiſtence of caloric as a diſtinct ſpecies of matter are derived from the obſervation of what has been called radiant caloric, and more eſpecially from the ingenious and curious experiments of Dr. Herſchell, on the ſeparation of heat from thoſe of light. This gentleman has found, that the priſm divides the ſolar beams not only into different coloured rays of light, as has been long known, but alſo into inviſible rays of heat, which are governed by peculiar laws in their refractions and reflections. This diſcovery, both unexpected and curious, has ſerved to convince ſome philoſophers that beyond all doubt caloric is proved to be a peculiar and diſtinct ſpecies of matter, ſeparable and now ſeparated from all other ſubſtances. It is not, however, altogether finally deciſive of this queſtion; and it may perhaps be worth while to conſider, how far the probability of the material nature of caloric is increaſed by theſe experiments.

It was on the ſide of the ſpectrum next the red ray that the rays of caloric were found. On the oppoſite ſide next the violet ray certain other rays have alſo been diſcovered, which do not produce heat, but which have a greater power of reducing metallic oxydes than the rays of the ancient ſpectrum. Theſe rays, as well as thoſe of caloric are wholly inviſible, and their exiſtence is inferred only from ſome chemical properties which they appear to poſſeſs.

Mr Leſlie, in a very ingenious work upon Heat, has lately turned his attention to this ſubject. He

has with great induſtry and ſagacity performed many curious and valuable experiments, the reſults of which are far from coinciding with thoſe of Dr. Herſchell. He has certainly ſhewn that the radiation and abſorption of heat are guided by laws eſſentially different from any before imagined, and the facts which he has diſcovered are in themſelves highly intereſting, independent of all deductions from them. But his object has been to diſprove the exiſtence of radiant caloric, and to ſhew that while heat and light are eſſentially the ſame, heat is not at all conveyed from hot bodies to colder ones in rays, but that the air is the vehicle of conveyance, and is affected by pulſations from the ſudden rarefaction of the portion next the hot ſubſtance, in a way not altogether diſſimilar to that in which the ſame medium is influenced by the vibrations of any ſonorous body. It cannot be denied that he has given conſiderable probability to his aſſertions, and that his experiments and theory appear to agree in many points. Yet the ſubject is by no means completely elucidated. Mr. Leſlie has partly endeavoured to invalidate, partly to doubt Dr. Herſchell's experiments. In his attempts to effect the laſt of theſe, every one will not be diſpoſed to join, and Dr. Herſchell's inveſtigations muſt receive too much ſupport from his known ingenuity in other branches of phyſics, to be combated in any other way than by experiments directly contradicting thoſe before the public. Mr. Leſlie, however, has ſhewn ſome cauſe to heſitate,

before the propoſed inferences be admitted. There are other rays beſides thoſe directly from the ſun, which paſs through the priſm, and are to be found beyond the red ray. Something ſhould be allowed for reflection, and ſomething, as Mr. Leſlie thinks, for inaccuracy. The effect of a ſtream of air, in arreſting the progreſs of radiant caloric, is alſo very remarkable, and much againſt Dr. Herſchell's hypotheſis.

The notion of inviſible rays of heat has been long entertained: the novelty of Dr. Herſchell's theory conſiſted in placing them among the ſolar beams, as ſeparate from the rays of light. It is clearly ſuppoſed that the inviſible and heating rays are pure caloric. If it could be imagined that they were any modification of light, we ſhould be diſpoſed to account for the appearance of heat in the old way, at leaſt no proof of the contrary doctrine would have been adduced. Dr. Hutton and other philoſophers have doubted as to the neceſſity of viſibility to the exiſtence of light, and, as it ſeems to me, on very good grounds. It is extremely probable, that either the air or the combuſtible ſubſtance contains light inviſibly, which the proceſs of combuſtion afterwards developes The compoſitions called pyrophori can be deprived of their power of emitting rays of light by expoſing them to ſevere cold. This luminous quality can however be reſtored to them, by a very moderate riſe of temperature. In this caſe, if light and heat be really different, it can ſcarcely be doubted

that the light exiſted in the frozen pyrophorus in an inviſible ſtate. Hence, it is only light in a peculiar ſtate that can be ſeen. A certain intenſity of light, moreover, is neceſſary for exciting viſion. It is well known that ſome animals can ſee diſtinctly where the human eye has no perception. The human eye itſelf varies in its power in different perſons, and even in the ſame perſon, under different circumſtances. Viſibility therefore is not indiſpenſable to the exiſtence of light, but only to a peculiar mode of its exiſtence. Light we certainly know to be ſuſceptible of numerous modifications, as manifeſtly appears from the various coloured rays of which the ſolar beam is compoſed. Theſe rays differ eminently in their powers of heating, no leſs than in their other properties; and their phenomena ſuggeſt the well-grounded ſuſpicion that the chemical affections of light are yet but imperfectly underſtood. Light is tranſmiſſible through certain bodies, called from that property tranſparent. But radiant caloric is not tranſmitted through all theſe ſubſtances. Some, ſuch as glaſs, refuſe a paſſage till they are themſelves ſaturated with it. Even the air, which tranſmits caloric from fires and other chemical combinations, is itſelf affected by the heat, as Mr. Leſlie's experiments clearly ſhew. But the ſolar rays of radiant caloric pervade air like the rays of light themſelves, leaving no trace of their progreſs. One cannot admit the alleged facts as poſitive proof of the material nature of caloric, after ſuch reflections. If light and heat

be indeed ſeparate bodies, perhaps their mutual combinations may ſerve to alter and diſguiſe each in a ſufficient variety of ways, to account for every modification undergone by either.

The caloric can ſcarcely be all radiated from a cooling body, nor even the greater part of it: ſince in that caſe hot bodies would cool faſteſt in the rareſt media, or in a vacuum, and in air ſooner than in iron filings or in mercury. But the very reverſe is the fact. Hence there muſt be a power of attraction in the receiving body, as well as one of repulſion in that emitting caloric. The cold reflected by mirrors in Pictet's and Leſlie's experiments is referable to this head, and is certainly a moſt curious phenomenon. Dr. Hutton accounted for this by ſuppoſing that the temperature of all bodies is kept up by the radiation from ſurrounding objects, and that of conſequence the vicinity of cold bodies, which radiate leſs heat, would be equivalent to the ſubtraction of heat from the thermometer uſed. And this certainly does explain the depreſſion of temperature, in as far as it may be occaſioned by the mere approximation of cold bodies; but by no means, why this depreſſion ſhould increaſe, when by the reflecting mirror more radiant heat is thrown on the thermometer. One would think that the temperature ought to be raiſed by ſuch a direction of the rays of heat. For though the mirror concentrates into one focus, upon the bulb of the thermometer, many rays from the ſurrounding bodies, which rays would

not otherwiſe ſtrike that inſtrument, the thermometer does by no means make an equal return, but at moſt can only radiate a few rays to the mirror, proportioned to the minuteneſs of its maſs, and the nature of its ſurface, now proved to have little diſpoſition for plentiful evolution of calorie. In fact, it ſeems neceſſary to allow, that ſome poſitive cold is directed by the mirror, and I feel myſelf diſpoſed in theſe circumſtances to liſten with a favourable ear to the ingenious account lately propoſed of this, and ſimilar phenomena, by Mr. Leſlie and Count Rumford. Mr. Leſlie's experiment, in which he found that agitation of the air, through which heat is radiating, materially affects the paſſage of the ſuppoſed rays, is indeed very ſtumbling to the adherents of the oppoſite theory. At the ſame time the paſſage of heat through the Torricellian vacuum is a little in the way of Mr. Leſlie's theory, unleſs we grant that the ſuppoſed vacuum is a plenum occupied by vaporiſed mercury, which may be ſuppoſed to perform the office of air, in the conducting of heat. There is an experiment eaſily made, which may be uſed to illuſtrate the conveyance of heat ſuddenly to ſome diſtance by heated air. If any hot piece of iron or ſteel, (the point of a large pair of common ſnuffers anſwers very well) be covered with liquid tallow, in which there float any ſmall black ſubſtances to ſhew the motion of the fluid matter, and the iron be then ſuddenly brought in contact with the flame of a candle, the portion of melted tallow next the candle is imme-

diately thrown with ſome violence through the colder tallow, probably nearly as the heated air is repelled from the hot ſubſtance, according to Mr. Leſlie's hypotheſis. Another curious circumſtance may be remarked in the ſame experiment now related. If the point of the ſteel inſtrument be retained for ſome time in contact with the flame, the whole of the fluid is repelled to ſome diſtance, leaving the hotter part of the iron perfectly uncovered.

Beſides theſe conſiderations which muſt induce us to heſitate, before we admit as proved the diſcovery of the ſolar calorific rays, there is another fact which ſeems to me to increaſe the doubt upon this ſubject. Meſſrs. Wollaſton, Ritter, and Bockman have demonſtrated, that on the oppoſite ſide of the ſpectrum, beyond its verge, certain ſubſtances, as muriate of ſilver, placed there, ſuffer a more rapid de-oxydation than in any other ſituation. From whence it is inferred, apparently with juſtice, that there muſt exiſt in this ſpace ſome influence connected with the ſolar rays, and this they conceive to ariſe from ſome other inviſible rays accompanying the common rays, but differing from them in refraction. That theſe rays are not rays of caloric we certainly know. But they are ſufficient to prove that all the ſolar rays are not viſible, and that the quality of inviſibility does not afford ſufficient grounds for denying the preſence of rays which are not heat, and which may therefore exiſt in the calorific rays in diſpute.

It has alſo been obſerved that a thermometer of

which the bulb is blackened ſhews a greater increaſe of temperature when the calorific rays are directed to it, than a thermometer with a ſhining bulb does in the ſame ſituation. In this circumſtance there appears much analogy with light, and but little with heat. Black bodies no doubt, and dark coloured bodies in general, have a greater power of abſorbing light. But as far as I know, it has never yet been aſſerted that caloric ſhews any preference to particular colours. And if it ſhall appear by further experiments that the radiant heat follows the ſame laws in abſorption as light does, we ſhall have ſtill further reaſon no doubt whether the calorific rays are pure heat.

I wiſh it to be obſerved that I am far from objecting to the truth, or undervaluing the merit of Dr. Herſchell's experiments. I have only endeavoured to queſtion the certainty of an induction from theſe experiments, the truth or falſehood of which, however, does not neceſſarily affect that of the experiments themſelves. One may be induced then, for the reaſons which have been aſſigned, to remain in doubt concerning the materiality of caloric, and eſpecially of the juſtneſs of the recent arguments adduced to prove it. It may be allowable to aſſert that there are at leaſt three varieties of the ſolar ray: that the central rays of the ſpectrum contain viſible light and heat; that the calorific rays contain heat, but not viſible light; and that the laſt diſcovered kind contain neither of theſe. It may be aſſerted

that there are altogether nine modifications of the ſun's beams, each, certainly, in ſome reſpect differing from the other. And ſo far the proceſs has not departed from the road of fair induction. But when we advance beyond theſe limits, and affirm that the calorific rays are pure caloric, it ſeems to me that we preſume too much on that ſlender knowledge of caloric, which even the ſucceſsful labours of ſome of the ableſt men of the laſt hundred years has procured, of that moſt difficult, moſt curious, and yet moſt obſcure ſubject. Upon the whole, the hypotheſis of the materiality of heat, which can be ſupported by many ingenious arguments, and which is highly convenient for the explication of various chemical phenomena, yet ſeems to want ſomewhat of the clearneſs of complete proof.

Another theory has repreſented caloric as wholly immaterial, and as conſiſting of certain vibratory motions among the particles of matter. This notion has probably taken its origin from the obſervation of a very common fact, that all ſolid bodies produce or collect heat, when ſubjected to friction. In ſome caſes this production of heat is accompanied by a change in the ſubſtance producing it, as when iron is hammered, as it may eaſily be, to incandeſcence, which can only be done once. In other caſes this does not happen, as in the caſe of two pieces of wood, which will become hot as often as they are violently rubbed againſt each other. Theſe and ſimilar facts naturally enough ſuggeſted this theory, that caloric

than any of the preceding. This hypotheſis aſſumes the identity of light and heat. It ſuppoſes that light becoming united with matter exhibits the phenomena of heat. It is well known that all ſolid bodies ſhine at a certain temperature. They cannot by any means be heated above this point without ſhining. But what is the ſource of this light? It does not ariſe from the fire employed. For hot air, which itſelf is wholly inviſible, is yet able to communicate to a ſolid body, as iron, the property of emanating light. Nor is air at all neceſſary for this effect, ſince iron is ſimilarly affected in melted lead. Neither can the light ariſe from the ſhining body itſelf. For if this were true, by continuing the proceſs ſufficiently long, the ſolid ſubſtance would at laſt loſe its property of ſhining, having given out all its ſtock of condenſed light. But, on the contrary, however long a body is made to ſhine, it appears ſtill as bright as at firſt, even after an interval of months. If then the light emanated from a hot body was not contained in it when cold, and does not proceed from the fire, at leaſt in the form of viſible light, what other ſource is left for it? Plainly none, unleſs the caloric of the air, or other ſubſtance communicating heat to the ſhining body. Whence the inference ſhould be, that caloric of a ſufficient intenſity is radiated from hot bodies in the form of light: in other words, that light is caloric. Heat in almoſt all caſes produces light, and light, in return, very frequently produces caloric. If inſtead of ſaying fre-

quently, we were borne out by facts to say always, there would remain very little doubt of the identity of light and heat. But this theory does not want, more than those already stated, its own share of difficulties. It has not been found possible, hitherto, to heat gases to the shining point. But these bodies ought to shine as well as others, if heated to the proper degree. Whether they require a very high temperature to enable them to shine, and are merely to be considered as bad radiators of heat or light, future experiments must determine. It is clear, however, from the ingenious experiments of Mr. Leslie, that bodies do vary in that quality very much from each other. On the other hand, though the solar beam, and the rays of terrestrial fires, excite heat in the bodies on which they fall, the rays of the moon can by no means be shewn to produce a similar effect. This has been attributed to the extreme tenuity of the lunar rays, which are supposed to be many thousand times more rare than those of the sun. Whereas, the highest powers of convex lenses have not been able to concentrate these rays so much, as to render it likely in theory that heat should be so produced.

If these two objections could be obviated, the hypothesis of the identity of light and caloric would be considered as the most probable yet proposed, concerning the nature of caloric.

These are the principal, or most probable theories, which profess to explain the phenomena of heat.

From the number of arguments which can be adduced for the ſupport of each, one inference may be not unfairly drawn: That philoſophers are yet very far from being completely acquainted with the intimate nature of the ſubject of the preſent inquiries; and that none of the hypotheſes is free from ſolid objections. In reality, the ſubject is by no means completely inveſtigated. But though the cauſe thus eludes our efforts, conſiderable progreſs has been made in arranging and explaining the facts; many curious and important phenomena have been diſcovered and inveſtigated, and chemiſtry, with its dependant arts, has received ſolid benefits from the experiments and reſearches which have been undertaken for the purpoſe of eſtabliſhing or confuting one or other of theſe theories.

The production or collection of caloric by friction is an extremely curious phenomenon. In thoſe caſes in which the body remains unaltered, as to what is termed its capacity for heat, and in thoſe where it does not affect the ſtate of the atmoſpheric air, there are great difficulties in every way of accounting for the riſe of temperature. Count Rumford has ſhewn, that iron, expoſed to violent friction, will become exceedingly hot, even under water, though there appears no cooling of the ſurrounding bodies. Caloric indeed may be here ſaid to have flowed in from the ſurrounding matter, the place of each portion being immediately ſupplied by the caloric behind, in a ſimilar manner to that by which water

ruſhes to replace any portion removed, each particle occupying ſucceſſively the ſituation of that before it. In this caſe caloric may be ſuppoſed to be collected by friction, as electricity is on the ſurface of glaſs and other electrics by ſimilar means. It might be curious and would not probably be very difficult to try whether the conducting powers of bodies for caloric have any relation to the increaſe of their temperature by friction. Perhaps if a body could be as completely inſulated from caloric, as it can be from electricity, friction would not heat it.

Although it may be difficult to point out with due preciſion the exact nature of the ſuppoſed motion amongſt the particles of bodies which produces the phenomena of caloric, yet we are ſtill too little acquainted with the properties of matter to be able to deny the poſſibility of ſuch a motion. It is generally ſuppoſed, that the ultimate particles of bodies do not touch each other, for proof of which it is aſſumed that almoſt all ſubſtances contract their bulk when their temperature is diminiſhed. But if the particles of matter were already in actual contact, they could not approach any nearer to each other. And as the definition of an ultimate particle ſuppoſes a portion of matter which no longer admits of deviſion or penetration, the particles themſelves cannot be ſuppoſed to dilate or contract their dimenſions. Hence ariſes the inference that the particles of bodies do not touch each other. In this argument, however, there are ſome objectionable

points. The definition of a particle which denies the poſſibility of its contraction or dilation is itſelf without proof, and is moreover not very ſuſceptible of any. It cannot be a very eaſy taſk to ſhew that there are ultimate particles which are not themſelves diſtenſible by caloric, but amongſt which this ſubſtance or quality penetrates, and, either by its chemical or mechanical power, ſeparates them. But even if this were granted to be true, it does not ſeem a neceſſary conſequence that the particles do not touch. It certainly muſt follow that they do not touch at every point. It is probable, however, that a partial contact of particles might be made to agree tolerably well at once with the expanſion of bodies by heat, and with the impenetrability of the particles themſelves.

The connexion between caloric and light, if indeed theſe are different bodies, is very obſcure. But it is well known that moſt bodies ſhine, that is, emit rays of light, very copiouſly at an elevated temperature. It has been an object of curioſity to aſcertain the point of the thermometer at which this takes place. But in temperatures ſo high the mercurial thermometer cannot be applied for obvious reaſons. To ſupply this deficiency a rule was propoſed by Sir Iſaac Newton for calculating the progreſs by which hot bodies cool, and thence deducing the temperature at any period in the proceſs. That illuſtrious philoſopher imagined that the heat loſt by any body at a very high temperature in equal ſmall times are as the heat exiſting in it, rec-

koning the heat in the body equal to its exceſs above that of the atmoſpheric air in which it is placed. That is to ſay, taking the times in arithmetical progreſſion, the portions of heat loſt in theſe times would be in a geometrical one. Thus if the body were 64° hotter than the air, and in the ſpace of five minutes loſt 32 degrees; in the next five minutes it would loſe 16°; for 64 : 32 :: 32 : 16. And in the five minutes before the application of the thermometer it would loſe 64°; for 16 : 32 :: 32 : 64: In the ſame manner if a body 180° above the heat of the air were obſerved in five minutes to loſe 60°, or was only 120° above the temperature of the air, in the next five minutes it would loſe leſs in the proportion of 180 : 120: that is 40°, and the whole heat now remaining will be 80°. In the next five minutes it would loſe leſs in the proportion of 120 to 80, or 26⅔. And in the five minutes prior to the firſt obſervation it would loſe a quantity greater than 60°, in the proportion of 120 : 180 or 90, and in the five minutes preceding it would loſe 180 : 270, or 135, and ſo on. Now 26⅔, 40, 60, 90, 135, are terms in geometrical progreſſion. This is a moſt ingenious method of determining thoſe very high temperatures to which no thermometer can reach, and it is found by actual experiment to agree very nearly with the phenomena, the differences not being greater than muſt ariſe from unavoidable inaccuracies. The principle however is aſſumed. This method is of con-

nearly at this temperature requires, about a red heat to make it diſtil. But according to ſome experiments which he made, the degree of heat at which bodies appear luminous falls ſomewhat higher. He found that with a mercurial thermometer moſt accurately graduated, mercury boils at 672°. He next proceeded to try whether boiling mercury ſhone in the dark; every precaution was taken to enſure the accuracy of the experiment. In order to make the determination accurate, proper aſſiſtants were procured, ſome of whom were placed in a room perfectly dark, in which they remained for ſome time that the eye might become as ſenſible as poſſible. The room communicated with another by a hole in the wall with ſeveral turnings, ſo that no ray of light could poſſibly paſs from the one apartment to the other. Through this hole the mercury which was boiling in the light room, was handed into the dark one, for examination of its power of emitting rays of light, but it could not be at all perceived. From this it was inferred that the ſhining point was above 672°. The means, however, of determining whether a body ſhines or not at a certain temperature are not perfect. The only teſt of accuracy is the human eye, and it is well known that different eyes vary very much from each other in their power of viſion, and even the ſame eye at different times and in various circumſtances. When the mercury at 672° was introduced into the dark room, although the

perſons preſent did not ſee it, a cat, which was there, ſeemed to have ſome perception of it, walking round the veſſel in which it was contained. This however might have been a feeling of heat as much as a perception of light. The mercury in the thermometer may be ſuppoſed not to be a perfectly accurate meaſure of the degree of heat at which mercury boils, on account of the irregular expanſions of moſt bodies at high temperatures. But it appears a perfectly fair and reaſonable induction that at whatever degree of the thermometer mercury boils, the luminous point muſt be ſomewhat higher, at leaſt of that body, and probably alſo of all others, ſince there ſeems reaſon to believe that this effect of heat is produced in all bodies that are capable of it nearly at the ſame degree.

Since then it appears, that 635° propoſed by Sir Iſaac Newton is too low for the point of viſibility in the dark, it became an object of ſome curioſity and importance to arrive at a more accurate determination. Dr. Irvine found by numerous experiments, that equal bulks of iron and water of different temperatures being taken, and the iron immerſed in the water, the new temperature is *nearly* the mean between the former ones. He heated iron of a known bulk to a proper degree, it was then introduced into the dark apartment through the orifice already deſcribed, and it was allowed to cool till it juſt diſappeared. At that moment it was dropped into water of a given bulk and temperature, and

the heat was obſerved which it communicated to the water. If the quantity, by bulk, of water was ten times greater than that of the iron, he computed that the iron had been ten times hotter than the heat which it imparted to the water. That is, if the water had gained one degree, that the iron muſt have loſt ten degrees; or in other words let the water be taken as ten and the iron as one, and for every degree given to the water allow ten degrees to have been in the iron. Proceeding in this manner, Dr. Irvine made many experiments to aſcertain the luminous point, but I have not been able to learn accurately the numerical reſult.

In a ſimilar way he made many trials of the heat of a common coal fire, by examining the temperature of a piece of iron which had been allowed to remain in it for a conſiderable time. In no leſs than twenty ſucceſſive experiments, inſtituted to aſcertain this point, Dr. Irvine never found the reſult higher than 796°, or lower than 790°. This coincidence, which in experiments of this kind is truly remarkable, may be regarded as nearly exact, and adds much to the probability of the juſtneſs of the method employed. The ſources of inaccuracy alſo ſeem chiefly to have been ſuch as rather lower the reſult. Such for example as the ſteam, which might be formed, the heat neceſſary for which ought to have been added to raiſe higher the ſhining point. Such errors, however, in carefully conducted experiments, are not ſo great as might be expected, as will in

ſome meaſure appear from ſome calculations in the third part of theſe Eſſays, concerning ſimilar loſſes by ſteam. Dr. Irvine, by varying his experiments, illuſtrated the degree of accuracy to be attributed to this method. He determined the melting point of lead by the mercurial thermometer to be 594°. He allowed a piece of red hot iron to cool till it was juſt hot enough to melt a piece of lead put upon it, and then trying its temperature by immerſion in water, he found it not above a degree different from the thermometrical determination. In oppoſition to theſe concurring circumſtances, there is only one reaſon for ſuſpecting the point to be too high; that is, becauſe when any ſubſtance acquires ſoftneſs, it tends to fluidity, and abſorbs latent heat. But after all allowances for this ſoftneſs the point of ignition cannot be reckoned lower than 780°.

Dr. Irvine employed this ſame proceſs to aſcertain many lower degrees of temperature, ſuch as the melting points of tin, zinc, &c. And he continued to remark a ſurpriſing and gratifying coincidence between the reſults thus obtained and the ſame points when he examined them by the teſt of the mercurial thermometer. There ſeldom appeared more than three or four degrees of difference between the two modes of obſervation and computation.

Caloric is well known to have a ſtrong tendency to diffuſe itſelf in all directions, nor can it be retained in any body otherwiſe than by the juxta-poſition of other ſubſtances ſtill more loaded with heat.

Although caloric has been clearly proved not to enter into all bodies with equal facility, or in the ſame quantity, yet a hot and a cold ſubſtance in the immediate vicinity of each other very ſpeedily acquire the ſame temperatures. This property has been diſtinguiſhed by the name of the equilibrium of heat, and various attempts have been made in this iſland and by our continental neighbours to invent ſome hypotheſis which might afford an explanation of it. Caloric has been ſuppoſed to be a fluid of a very ſubtle nature pervading all matter, as water penetrates the vacuities of a ſponge; and hence, when accumulated in larger quantities in any particular body than in thoſe ſurrounding it, that there was a tendency in the caloric to flow out of the hot body into the colder ones. This tendency, however, certainly cannot be ſuppoſed to differ from an acting force; for if heat be accumulated in any body it ought to remain there for ever, unleſs either attracted or repelled from its ſituation. The caſe of a ſponge floating in water, uſed for the illuſtration of this opinion, undoubtedly ſuppoſes a poſitive power. For if water could be accumulated in the ſponge, it would only leave that body, being attracted by other bodies, or by the gravity of the earth, or by ſome ſuppoſed elaſticity of its own ſubſtance. This theory was altered and explained by Pictet and others, who attributed to the particles of caloric the power of repelling each other, which repulſion increaſes directly as the diſtance diminiſhes between the particles. From this it follows

that caloric is retained by one force, that of the attraction of the body in which it is contained, and that it is diſcharged by another, that, to wit, of the mutual repulſion of the particles of caloric. Conſequently when two equal bodies are placed in the vicinity of each other, and the force of the attraction of one for heat plus the repulſion of the particles of caloric in the ſecond, equal the force of the attraction of the ſecond for heat, plus the repulſion of the firſt; theſe two bodies will not communicate heat to each other, that is to ſay, they are of the ſame temperature. Another opinion is that of Prevoſt, who dropping the conſideration of the attraction of bodies for caloric altogether, attributes the equilibrium of heat ſolely to the repulſion between its particles, or, as it is now uſual to ſtyle it, to the radiation of caloric. This radiation is imagined to be greater or leſs exactly as the temperature is higher or lower. Bodies therefore mutually heat each other, and temperatures are determined by the difference between the rays given out and thoſe received. Hence clearly an equilibrium of caloric muſt ariſe. But this theory ſeems to me wholly inconſiſtent with the well aſcertained fact that hot bodies cool ſlower in a vacuum than in the open air. Caloric therefore muſt be conducted from one body to another, not by radiation only, and bodies muſt ſtill be allowed to conduct caloric from each other without the intervention of radiation.

This theory of Prevoſt has much analogy with the

ingenious Hutton's opinions on the ſame ſubject. Dr. Cleghorn alſo appears to have viewed the phenomena of caloric in a manner extremely ſimilar to the hypotheſis of Pictet. Upon the whole, it may be remarked, that none of theſe theories are of extremely recent invention, unleſs in ſome few points, where additions or corrections have been made. The laſt hypotheſis, which would attribute the whole motions of caloric to radiation, has been lately oppoſed in a ſeries of new experiments by Mr. Leſlie. In particular, he has obſerved that the agitation of the air through which the caloric is radiated very much impedes its progreſs, a fact correſponding with his own opinions and illuſtrative of the carrying power of air, but not eaſily reconcileable with the previouſly entertained ideas of radiation. On this ſubject however it is perhaps not yet poſſible to decide with propriety till other philoſophers have repeated theſe *experimenta crucis*, or till the adherents of the doctrine of radiation have had time to inſtitute other trials to determine the truth of theſe new opinions.

If it ſhould be allowed that every additional doſe of caloric with which any body combines is held by a weaker affinity than that before admitted, which muſt be true if the chemical union of heat with matter be granted at all, and if the particles of caloric repel mutually the more ſtrongly as they approach nearer to each other, a queſtion ariſes, can caloric be added to any body *ad infinitum*? Are there any bounds to the poſſible augmentations of temperature?

It appears reaſonable to ſay that there muſt be a limit. For the attraction of the body for caloric is conſtantly diminiſhing, and the repulſion between the particles of caloric as conſtantly increaſing. That is to ſay the means which enable the body to take in caloric, are taken from it gradually, and the power of the caloric to eſcape from confinement are progreſſively enlarged. There muſt therefore be ſome point at which no more heat can enter, ſome point at which an equation ſhall be eſtabliſhed, the two oppoſite forces mutually balancing each other. A body at this temperature can no longer admit heat, and cannot even be retained at the ſame point, unleſs all other bodies were equally warm. For though ſurrounded by any number of equally hot ſubſtances, if there exiſted one colder body without the circle, the caloric, however gradually, would infallibly be diſperſed in that direction. It does not ſeem likely that any practical uſe could be made of this inference, admitting it to be juſt. If, however, any experiments could be deviſed, by which there could be determined the ratio in which the attraction of any body for ſucceſſive doſes of caloric diminiſhes, and on the other hand the ratio in which the mutual repulſion of the particles of caloric increaſes, perhaps we might be enabled to calculate the higheſt degree of heat to which that body would be raiſed under the influence of theſe two powers. A third power, no doubt, might be conceived to poſſeſs ſome ſhare in the arrangement. I mean the force by which caloric might be repelled from a very hot

ſubſtance into one at a lower temperature, the ſtrong repulſion of the caloric in the hot body overcoming part of the weaker repulſion of the cooler. This, however, would have its limits, which might be determined on the ſame principles as before mentioned. If we could conceive caloric to be ſo condenſed that its repulſive powers would exceed thoſe of the caloric in a body at the higheſt degree of heat, computed as has been propoſed, a body ſurrounded by ſuch caloric would be forced to admit it in greater quantity, and by thus ſuppoſing *à priori* that caloric is ſo condenſed, there would be no limit of the entrance of heat into a body. If, however, the repulſion of the particles of caloric increaſe inverſely as the ſquares of the diſtances, or in any other ratio, the condenſation of caloric muſt have a limit, and nothing would account for a higher temperature but the gratuitous ſuppoſition of a body already ſo heated by unknown means.

If there were any certain foundation for ſuch a calculation as has been now imagined, which I am far from ſuppoſing to be the caſe, it would be poſſible, by inverting the proceſs, to determine alſo the loweſt degree of heat to which any body could be cooled. But even if all this were juſt in theory, there would be little reaſon to hope that we ſhould ever be enabled to procure practical reſults, or to compute with due accuracy either the ratio according to which bodies have their attraction for caloric diminiſhed, or that by which the particles of caloric proceed to increaſe their repulſion for each other.

ESSAY II.

ON SOME OF THE PRINCIPAL DISCOVERIES MADE BY HELP OF THE THERMOMETER.

No branch of human knowledge has received more extenſive additions from the invention and uſe of an inſtrument calculated to facilitate and improve the methods of obſervation than the ſcience of heat has derived from the employment of the thermometer, under the influence of which the progreſs of this department of philoſophy has been wonderfully accelerated, and the theories of the nature of caloric ſubjected to the ordeal of a ſevere and accurate reference to experiment. The properties of caloric indeed were little attended to or underſtood before the invention of thermometers. All that was known concerning it was derived from the obſervation of its effects upon the human body;

a ſource of knowledge liable to great inaccuracies from various cauſes. The ſame ſubſtance, by a very ſimple experiment, may be made to appear hot to one hand, and cold to the other. And in many diſeaſes, where the patient has a ſtrong ſenſation of cold, it frequently happens, that the body is then warmer than it ever is in a healthy ſtate. The very ſenſe of feeling too is liable to be deſtroyed, at a temperature a little above or a few degrees below that of the human body. Hence all ſuch obſervations are neceſſarily confined within theſe limits. This meaſure of caloric, therefore, is merely relative; whatever is warmer than the body at the time appearing hot to it, and whatever is colder appearing cold. And as the heat of the hand, the principal organ by which determinations of this kind are made, is perpetually varying, the ſubſtance examined is pronounced hot at one time and cold at another, though its temperature be really the ſame in both caſes.

The invention of the thermometer ſuperſeded all the former vague and uncertain methods of eſtimating the degrees or qualities of caloric. It ſhewed that all kinds of heat were the ſame, and differed from each other only in intenſity, an opinion very diſſimilar to that entertained formerly, when the grandeur or baſeneſs of the ſource was ſuppoſed to influence and alter the nature of caloric itſelf. Experiments alſo made in one country could be repeated and extended in another, when the thermometer was employed; and a degree of accuracy was introduced

into the philoſophy of heat, of which the ſubject had not been imagined to be ſuſceptible. For the conſtruction of theſe inſtruments, after various trials, the preference is now given to mercury; a ſubſtance which does not freeze but with extreme cold, nor boil till raiſed to a very high temperature, affording, thereby, an extended interval, for the menſuration of the degrees of caloric. Its coheſive attraction moreover being very great, it is free from the inconvenience of adhering to the ſides of the glaſs tube. Beſides which, its expanſions have been proved, by the experiments of Black, De Luc, and Crawford, to be, within certain limits, the beſt meaſure of heat that we are poſſeſſed of; that is to ſay, it expands equally with equal increments of temperature. Not that it is entirely or perfectly accurate, but that it approaches accuracy nearer than any other known ſubſtance, and that within the limits of its intermediate expanſions, and avoiding the temperatures which border on its freezing and boiling points, its errors are probably not of great practical conſequence. ¶

The computation of degrees ought to begin from the natural zero or point of privation of all heat. But this point not being ſufficiently known, the zero has been placed at the freezing point of water, by almoſt all the philoſophers of the continent, and by ſeveral of our own country. Notwithſtanding objections urged for ſome time, againſt the permanency of the freezing point of water, it ſeems upon the whole to be by far the moſt unexceptionable point for

the commencement of the thermometric ſcale. And it accordingly makes the zero in the thermometers of Reaumur and Celſius chiefly employed in Europe. Faſhion, more than any circumſtance of ſuperiority, has induced in this iſland the preference of a thermometer on the principles of Fahrenheit, who placed his zero at the greateſt degree of cold which he had been able to produce by a mixture of ſnow and common ſalt, and which was once ſuppoſed to be the greateſt poſſible cold. Theſe thermometers, however, are now univerſally conſtructed and graduated by the help of melting ice or ſnow, which is found a far eaſier and more certain method, marking 32 at that point. By the experiments of Mr. Walker, of Oxford, however, it appears, that the thermometer has been ſunk ſeveral degrees below Fahrenheit's zero by the mixture of ſnow with ſea ſalt;* and long before Mr. Walker's experiments, Dr. Irvine had produced in this way, a cold equal to —9 of Fahr.

About the middle of laſt century, by help of the inſtrument, the merits of which we have been diſcuſſing, ſome obſervations were made and ſome conſequences deduced, which have produced unforeſeen

* This experiment was performed in the following manner: "the temperature of the air being 34°, an ounce of ſea ſalt at 34° "was mixed with fourteen drachms of ſnow at 31°, the temperature "produced was —7; much of the ſalt was ſtill ſolid. I poured off "the clear fluid, and mixed more ſnow at 28° with the moiſt ſalt; "the temperature produced was —7. Repeating this experiment "with a greater quantity of ſnow and ſalt, the cold was —9."

and immenſe changes in the whole ſcience of chemiſtry, and conferred immortal honour upon the illuſtrious diſcoverer. Dr. Black, to whom this merit belongs, made theſe experiments and deductions about the year 1755. M. De Luc has claimed an equal ſhare of the honour of theſe diſcoveries, to which, however, if we believe the ingenious Dr. Robinſon, in his notes upon Dr. Black's Lectures, he has very little title. It has been long ago obſerved, that ice, when it is heated to 32 Fahrenheit, ſuddenly ſtops in its riſe of temperature, though the temperature of the ſurrounding bodies be higher than 32, and pertinaciouſly continues at the ſame point till the whole is melted. No intelligible explanation, however, had been offered of this fact, which indeed, in a great meaſure appears to have eſcaped the notice or to have attracted very little of the attention of philoſophers. It might be demanded, did the melting ice admit heat or not? If it did not, what ſtrange cauſe prevented the caloric from entering into the ice at that point alone? If, however, it did, how are we to explain the admiſſion of caloric during ſo long an interval without any correſponding riſe of temperature. It would appear, either that nobody thought of anſwering theſe queſtions at all, or at leaſt that nobody was able to propoſe a juſt and ſatisfactory account of the matter. Such was the ſtate of philoſophical opinion concerning this ſubject at the time of Dr. Black's firſt attention to it. That gentleman, having conſidered the ſtate of the theories and knowledge

of caloric at that period, reſolved to ſubmit to the teſt of experiment, whether heat did or did not enter into melting ice. And he clearly ſhewed that the ice did not refuſe admiſſion to the caloric as ſome imagined, but on the contrary, received an unuſually large quantity. He obſerved that a veſſel with ice, though it did not acquire a higher temperature, while any part of the ice was unmelted, certainly admitted heat, ſince a ſtream of cold air deſcended from the veſſel during the whole proceſs of fuſion. He obſerved further, that a pound of ice and a pound of water each at 32°, being each ſeparately mixed with a pound of water at a higher temperature, that the mixture of the two pounds of water had a temperature nearly the mean of their former temperatures; but, that when ice was uſed as one of the ingredients, the temperature of the mixture was lower than the mean, and that 140° ſeemed to have diſappeared, or to have entered into the ice during its converſion into water. Theſe 140 degrees were ſtyled by Dr. Black latent heat, becauſe, according to his view of the ſubject, the thermometer gave no indication of their preſence, which was only to be diſcovered by the change of form from the ſolid to the fluid ſtate. This great diſcovery opened a new and prodigious field for the labour of philoſophers. Dr. Black ſoon ſaw reaſon to extend his doctrines of latent heat to all caſes of fuſion; and Dr. Irvine, then his pupil, experimentally determined its truth in ſeveral inſtances. He found the latent heat of ſpermaceti to be about 145°, as

meaſured by the capacity of melted ſpermaceti; that of bees' wax about 175°, and that of tin 500°, as meaſured by the capacity of ſolid tin. Dr. Black applied the ſame method of explanation to vaporizations of every kind, and ſhewed the neceſſity of large ſupplies of heat to the exiſtence of ſteam, and other bodies in a ſtate of vapour, by experiments convincing and unexceptionable, but which it is now needleſs to detail, when the fact is univerſally admitted. Dr. Irvine by experiment, made the latent heat of ſteam amount to about 800°, by a method ſuch as is detailed in Dr. Black's Lectures, though ſome particulars do not altogether coincide with the manuſcript account of the experiment in my poſſeſſion; Mr. Watt afterwards computed the latent heat of ſteam to be from 900° to 950 degrees; M. Lavoiſier ſuppoſes it to be as high as 1000 degrees.

Dr. Black thus eſtabliſhed the truth of a general and moſt important law of nature, that all bodies require for their fuſion and evaporation an immenſe, and formerly unſuſpected quantity of caloric. To the knowledge of theſe facts, it cannot be doubted, that we owe much of the modern advancement in chemical theory.

It has been already mentioned, that all known bodies contain, and more or leſs freely tranſmit heat. This fact is indiſputably true. But when it is next inquired, if they contain equal quantities in equal bulks, or in equal weights, or in any other way, the anſwer is not ſo clear. It was ſuppoſed by Dr.

Boerhaave, that bodies contain heat in proportion to their ſpecific gravities. But Dr. Black ſhewed this opinion to be unfounded, and that bodies had powers of containing heat peculiar to each peculiar ſubſtance, and that not connected either with the comparative weights or bulks of the ſubſtances examined. This power of being unequally heated by equal quantities of heat, has been the object of conſiderable inquiry, and was diſtinguiſhed by Dr. Black and Dr. Irvine nearly about the ſame time, by the term capacity. The ſame meaning has been expreſſed by various other terms, as comparative and relative heat, ſpecific heat, equilibrium, &c. If any body be heated a certain number of degrees by a given quantity of caloric, and its capacity be called 1.000, and another body be heated twice that number of degrees by the ſame quantity of caloric, then its capacity muſt be expreſſed by .500, or be only one half of the former, becauſe each degree of its temperature is raiſed by a portion of caloric only half of that which is neceſſary to raiſe each degree of the others. The capacities of bodies may thus be numerically compared, and this is generally done by calling the capacity of water 1.000, and comparing that of all other bodies to it. It is not poſſible to apply directly to any body a certain quantity of caloric to make this trial, for caloric is inſeparable from matter, unleſs the caſe of radiant caloric be conſidered as an exception to this, and even then, it is impoſſible to apply any meaſured quantity of caloric to a body. But it is

eaſy to obſerve, how much heat, equal quantities of different ſubſtances ſubtract from a given quantity of any matter, water for example, to have their temperature raiſed to the ſame degree. And this is in reality, to do the ſame thing, as if we could ſeparate the caloric from all other matter, and apply it in meaſures or weights to the ſubſtances under examination.

The diſcovery of theſe properties of caloric, has intirely altered men's opinions on that ſubject. New roads have been opened for the progreſs of philoſophical inquiry, and a degree of preciſion, and of truly uſeful and elegant knowledge, has been obtained, which there was formerly no reaſon to have expected.

All who have followed the footſteps of Dr. Black, in adding further to the facts or the theories concerning caloric, have begun by acknowledging the truth and the importance of his diſcoveries. As far as I know, no perſon has attempted to invalidate his obſervations, which are indeed ſo clear, and ſupported by experiments ſo ſimple and deciſive, that they enforce belief. Whatever additional facts may have been diſcovered, or whatever variation of theory may have been advanced, his merits cannot be thereby affected.

From the facts already ſtated, it appears that the fuſion, or melting of bodies, is not performed altogether in the manner that was at one time imagined. It is not enough, merely to add ſuch a quantity of heat as would have been ſufficient to raiſe the tem-

perature of the ſolid above the point at which fuſion takes place, but a much larger ſupply is required than is neceſſary for a ſimilar elevation of temperature either before or even after the liquefaction. In other words, a portion of caloric enters into the body that is fuſed, without raiſing its temperature. This quantity of heat, which in the caſe of water, amounts to about 140 degrees, for there is ſome difference of reſult in the experiments for aſcertaining this point, was called by Dr. Black, latent heat, and has received from ſome other philoſophers the name of caloric of fluidity. It was aſſerted by Dr. Black, to be the cauſe of fuſion, and to combine chemically with the ſolid, and form a ſubſtance differing both from the ſolid and the caloric, to wit, the fluid.

This opinion has been the cauſe of very great diſcuſſion, and was in particular by Dr. Irvine ſuppoſed to be, if a juſt, at leaſt, not a ſufficiently comprehenſive view of the ſubject. He was not diſpoſed to conſider the entrance of what is called latent heat into bodies as happening upon different principles from thoſe which always direct the operations of caloric upon matter. Though ready to admit the diſcoveries of Dr. Black, in all their extent and importance, and no man thought more highly of them, as none more fully appreciated their value, he imagined that latent heat was only a caſe of what occurred in every affection of bodies by heat, and that the caloric exiſted there preciſely in the ſame way as at other times, and could be diſcovered by the ſame

teſts which at any time give notice of its preſence. Not willing to deſcend to a diſpute concerning a term, he was ready to admit the phraſe, latent heat, as the expreſſion of a new and curious fact, though not without ſome modification in the exact ſenſe to be attached to it. He was accuſtomed to obſerve, that the latent heat followed as a mere conſequence of his peculiar view of the operations of caloric; by this aſſertion, not claiming any ſhare in the honour of diſcovering the exiſtence of latent heat, but expreſſing that his theory being granted, the previous diſcovery of Dr. Black fell into it as a part of a whole.

Dr. Irvine conſidering the phenomena of latent heat in compariſon and in combination with the diſcovery of the different capacities of bodies, was led to ſuppoſe that they might mutually illuſtrate each other, and that ſomething more might happen during fuſion than Dr. Black had imagined. All bodies, it appears, are differently affected by the ſame quantity of caloric. No two ſubſtances ſhew the ſame augmentation of temperature by equal increments of abſolute caloric. To raiſe any body from one given thermometrical point to another, requires an addition of heat, which muſt be greater if the capacity of the body be greater, and leſs if it be leſs: this quantity then is proportional to the capacity, as all experiments have ſhewn, as well in the loweſt as in the higheſt temperatures in which it has been poſſible to make the trial. If then we ſuppoſe that any ſubſtance were totally deprived of caloric, yet retained its power of receiving or com-

bining with it; it appears reaſonable to ſay, that a certain quantity of caloric applied to this body, would raiſe its temperature by a number of degrees proportional to its capacity. And, if this capacity were then increaſed, that a greater quantity of caloric would be required for producing the ſame effect or elevation of temperature.

To the beſt of my information, it was by a proceſs of reaſoning ſomewhat ſimilar to this, that Dr. Irvine was induced to conceive that it was poſſible that the capacity of water might be found to exceed that of ice; upon experiment being made, that if this really turned out to be the caſe, a new reaſon might be aſſigned why ice during its melting refuſed to admit of an augmentation of temperature. He imagined therefore, that a larger quantity of caloric might be neceſſary to raiſe the temperature of water to 32° than that of ice to the ſame degree, ſuppoſing the heating of each to begin at the point of total privation of all heat. In this caſe, the heat could not be called latent exactly in Dr. Black's ſenſe, ſince the thermometer ſhewed, that, though the number of degrees was the ſame in both inſtances, the value of each degree was different. All bodies therefore ſhould contain heat in a ſimilar way, and by a like mode of union, whatever that might be.

It is not often that I am able to expreſs in Dr. Irvine's own language, the circumſtances of his theory. But I now ſubjoin a paſſage from his manuſcripts explanatory of this ſubject.—"The reaſon why a body cannot be heated while

it is melting is, that it is then changing its capacity. From a ſubſtance that was eaſily heated, it changes to one that is heated with difficulty. All bodies in a fluid ſtate ſeem to be heated with more difficulty than when in a ſolid ſtate. Not that a body in a fluid ſtate tranſmits heat more ſlowly than when the ſame body is in a ſolid ſtate. On the contrary, it tranſmits heat better in a fluid than in a ſolid ſtate. But the ſame quantity of heat applied to the ſame body in a ſolid form, and in a fluid ſtate, will heat the ſolid a greater number of degrees than it will the fluid. Exempli gratia: if a quantity of abſolute heat 10 or X be applied to a ſolid body, and is capable of heating it 20°, reduce the ſolid to a fluid ſtate, and this quantity of heat will only be capable of heating it 10°. Therefore, if all the heat in the body in a ſolid ſtate was 20°, and if it became all inſtantly fluid, the whole ſenſible heat in it, in this caſe, would only be 10°; or the body would have become inſtantly ten degrees colder without any heat being taken out of it. And if the ſame body, in a fluid ſtate, became inſtantly ſolid, it would immediately become 10° warmer without any heat being thrown into it. In theſe caſes, the quantity of the heat in the body does not alter by the change of form. All that happens is, that the ſame quantity of heat produces a greater effect in heating in the one ſtate than it does in the other."

The firſt and indiſpenſable condition for the truth of this hypotheſis is to ſhew, that the capacities of water and ice, or of fluids, and of their relative ſolids

in general, really do differ in the manner here ſuppoſed. With this view, Dr. Irvine was at great pains, and made very many experiments to determine the capacities of ice and water. For this purpoſe, he uſed a third body to meaſure the heating or cooling powers of theſe two ſubſtances, ſometimes employing mercury, ſometimes iron filings, and laſtly, preferring fine river ſand waſhed, or pounded glaſs for the medium of compariſon: theſe experiments were conducted in the uſual manner, and with the uſual precautions, though much more than uſually embarraſſed by the different temperatures of the air, and the ſubſtances uſed. For it is impoſſible to perform experiments to determine the capacity of ice or ſnow without reducing that ſubſtance to a very low temperature, near the zero of Fahr. or a few degrees only above it. While the air, if many degrees higher in temperature, inevitably communicates during the proceſs a quantity of caloric, which tends greatly to the uncertainty of the reſults. But temperatures of the air near zero are very rare, and of very uncertain duration in our variable climate. More accurate reſults, therefore, might be reaſonably expected, if ſuch experiments were repeated in countries where extreme colds prevail, and where of conſequence, both longer time and better opportunity would be given for determining this point. From the means of many trials made at various favourable times by Dr. Irvine, he concluded the capacity of water to be to that of ice in a ratio not greater than 10 to 8.

Dr. Crawford found the proportion to be as 10 to 9 nearly, and Mr. Kirwan, if I do not miſtake, ſtated the numbers to be 16 and 9.

Conſiderable anxiety has been expreſſed to learn the exact manner in which Dr. Irvine conducted thoſe fundamental experiments, upon the truth of which ſo much of his theory depends. Many philoſophers appear to have imagined them to have been performed by a mixture of ice and water, which is altogether untrue. Such a thing may, perhaps, be conſidered as theoretically poſſible, but practically of extreme difficulty. Capacities are ſolely the expreſſion of the riſe or fall of temperature, produced by equal quantities of caloric upon different bodies. If, then, we could mix ice of a very low temperature with water, ſo that no water ſhould be frozen or ice melted, we ſhould have ground for determining their capacities for heat. The difficulty here would be to determine whether any ice was melted or not: other methods might be propoſed upon ſimilar principles, but equally difficult or impoſſible in execution.

Dr. Irvine had not recourſe to this proceſs. I have already given the outlines of his plan. He found the capacities of ſome ſuitable bodies, as river ſand, or iron filings, and compared them with that of water in the uſual manner. This being done, he uſed the ſame body to examine the capacity of pounded ice formed from diſtilled water, or of ſnow. The temperature of the room and veſſel was, in his experiments, always either 32° or below it; moſt commonly

conſiderably under 32°. He then took a known weight of ſnow or ice of a known temperature, in a veſſel of which the capacity was determined by experiment. Upon this he poured a certain quantity of river ſand waſhed, or iron filings of a certain temperature, with as much rapidity as poſſible: the new temperature was obſerved after ſtirring, and allowance was made for the heat gained or loſt: the temperature of the mixture was frequently 19°, 20°, 25°, 16°, &c. So that in a room where the air was below the freezing point, the accuracy of the reſult could not be affected by the formation of any water; ſtill there are many ſources of inaccuracy remaining. But in Dr. Irvine's hands, the capacity of ice always turned out to be leſs than that of water. In all his experiments, which were very numerous, and repeated with care for many ſucceeding years, he arrived at reſults approximating to each other, and concluded, to uſe his own words, that from the mean of all his trials, the capacity or relative heat of water to that of ice is not in a ratio greater than 5 to 4 or 10 to 8. To this I may only further add, that the near coincidence of the experiments of different philoſophers at different times and places, affords fair grounds for ſuppoſing that all of their determinations approach to accuracy. A praiſe the higheſt that any reaſonable man can ever expect to obtain for experiments of ſo delicate a nature.

In like manner, Dr. Irvine extended his theory to all other bodies whatever, and in ſome caſes deter-

mined and in all inferred, that it is a general law of nature that the capacity of all ſolids, for heat is increaſed by fuſion, and that of all fluids by vaporiſation. This law, the exiſtence of which was not before even ſuſpected, muſt be conſidered as an highly important obſervation, as all generaliſations of facts are to be regarded, and that whether the theory which is attempted to be deduced from it be ultimately eſtabliſhed or overthrown.

It appears then, that beſides Dr. Black's diſcovery of the great quantity of caloric neceſſary for the converſion of a ſolid ſubſtance into the fluid form, another remarkable alteration is produced in the habits of the body with regard to heat. It is changed from being a body, which is eaſily heated, or which requires only a ſmall quantity of caloric to raiſe its temperature by a certain number of degrees, to be a body difficultly heated or requiring a greater quantity of caloric for producing that effect. But if this change in the body could by any means be ſuddenly induced, it appears highly probable, if not abſolutely certain, that the temperature of the body muſt either immediately ſink conſiderably, or that a large portion of caloric muſt enter it to prevent that conſequence. Therefore, in whatever manner fuſion may take place, the fluid formed muſt, from the nature of the change of its capacity, immediately require a ſupply of caloric to enable it to remain only at the ſame temperature with the ſurrounding bodies. It is clear, that if a ſolid body be converted into a fluid of an

increaſed capacity, it muſt abſorb heat, which will exhibit the very phenomena that latent heat diſplays. Theſe arguments apply with equal force to the illuſtration of the circumſtances obſervable during the converſion of a fluid body into the aëriform ſtate.

If this period were not a little too early for introducing the explanation of the riſe or depreſſion of temperature produced in almoſt every chemical mixture, circumſtances illuſtrative of the preſent topic might be readily drawn from that ſource. Of theſe, however, we ſhall treat a little farther on. But, though the cauſe of liquefaction may perhaps bear much analogy to the changes mutually effected by chemical agents, it is not abſolutely neceſſary to refer to theſe facts for the explanation of this ſubject.

Various objections have been brought againſt this mode of explaining fuſion and vaporiſations. It cannot, however, at leaſt in my opinion, be ſaid to be altogether oppoſite to Dr. Black's explanation of the ſame phenomena, but rather is additional to it. Both theories admit alike, as the groundwork of their ſuperſtructure, the entrance of a large quantity of caloric during fuſion. Dr. Irvine's hypotheſis differs only in offering an explanation of the newly diſcovered fact, the enlargement of the ſpecific heat of the fuſing bodies, and in denying any peculiar or unuſual combination of caloric in theſe circumſtances.

Dr. Black, however, was not diſpoſed to allow the juſtice of this modification or alteration of his own

theory. He ſaid, that he ſuppoſed the capacities of ice and water really to be to each other as Dr. Irvine repreſented them, and certainly had no claim either to the ſuggeſtion or performance of the experiments made to determine theſe points. But the inferences did not appear to him to be correct. To quote the Doctor's own words:—"To this ſtatement (of Dr. Irvine's) I anſwer, that the alleged fact of diſparity between the capacity of ice and water, for heat may, indeed, be ſuppoſed to account for the thermometrical phenomena juſt now recited; but the principal fact or phenomenon is not accounted for by it, I mean the change of the ſolid into the fluid. Solidity, we all know, depends on adheſive attraction; but on what cauſe does fluidity depend? Will it be ſaid to depend on the abſence or ceſſation of coheſive attraction? I cannot imagine how coheſive attraction can entirely ceaſe, or be ſuſpended; or, if this ſhould happen, how it ſhould eaſily be again reſtored: nay, we have evident proof, from the round form of the drops of liquids, that it is not then entirely ſuſpended, but only weakened to ſuch a great degree, that the ſolid hard body is become a liquid. Now this is ſuch a violent change, that I cannot help thinking it muſt depend on ſome powerful cauſe. It may be ſaid, perhaps, that no other cauſe is neceſſary but the increaſe of ſenſible heat above a certain degree, together with ſome ſingularity in the nature of coheſive attraction, ſuch as that this attraction, though very ſtrong at certain ſmall and

imperceptible diſtances, becomes ſuddenly very weak, when we increaſe beyond a certain limit the diſtance of the particles of matter from one another, and this increaſe of diſtance is produced by ſenſible heat.

"If this were true, the increaſe and diminution of diſtance, by the action of ſenſible heat, ought always invariably to produce, each its appropriate effect, on the power of the coheſive attraction; as water when its degree of heat is above 32° is always liquid, ſo when its ſenſible heat is below 32°, it ſhould always be ſolid. But this we know is not always the fact. Water, in ſome circumſtances, can be cooled to 7 or 8 degrees below 32° without being congealed; and many other ſubſtances exhibit the ſame phenomenon, by retaining the fluidity in ſome circumſtances after their ſenſible heat is diminiſhed, conſiderably below their ordinary congealing point. And I now aſk, what is the cauſe which hinders the coheſive attraction from producing its effect, and changing the liquid into a ſolid? When I find by experience, that upon diſturbing ſuch over-cooled liquids, a quantity of heat is extricated from them, which did not appear immediately before, and that, while this heat is extricated, a proportionable part of the liquid congeals; I cannot help conſidering this latent heat which was in it, as having been the cauſe of its protracted fluidity."

This kind of reaſoning has appeared to Dr. Black, and to many other philoſophers, to be concluſive.

Yet it ſeems to me liable to ſome very conſiderable objections, which it ſhall now fall to my taſk, to ſtate as preciſely as I can. And firſt of all, it muſt be remarked, that the arguments, now recited, derive a great portion of their force, from the ſtatement given by Dr. Black, and ſome others, of Dr. Irvine's theory. This ſtatement, however, is by no means ſuch, as an advocate for that hypotheſis would chuſe to reſt his opinions upon. From cauſes already ſtated, I have not always been enabled to ſtate my father's doctrines in the preciſe terms in which he was himſelf accuſtomed to announce them. But it appears to me to have been a miſconception on the part of thoſe, who have reported his ſentiments, to ſay, that the capacity of the ice is firſt enlarged, and then a quantity of caloric admitted, diſtinguiſhed by Dr. Black by the appellation of latent heat. Such a ſtatement at leaſt is by no means neceſſary for the ſupport of other parts of his theory, and is an awkward incluſion of more points in the explanation, than are by any ſuppoſition required. I am at leaſt firmly perſuaded, if this were not ſo, and if Dr. Black's ſtatement were correct, that notwithſtanding we are not driven from our ground, but are merely compelled to alter a minute tract of our previous argument. This theory, therefore, may be defended in two ways, either by admitting Dr. Black's as the true ſtatement (and againſt this almoſt all objections have been directed), or by taking the account which I have given. That account I briefly recapitulate.

The ſolid differs from its relative fluid, when both are of the ſame temperature, in theſe circumſtances, merely, that the capacity of the former is leſs than that of the latter, and that of conſequence, the heating of both beginning at the natural zero, more caloric is neceſſary for the elevation of the temperature of the water, than for that of the ice. The difference between the whole heat in water at 32°, and the whole heat of ice at 32° is called the latent heat of that body, and ice being converted into water requires this quantity of caloric to retain its temperature at the ſame degree as before. But this caloric does not enter the ice before its capacity is changed. Much leſs is the capacity enlarged before the caloric enters the body. Theſe events are ſynchronous, and are neither cauſe nor effect of each other, but are mutually the conſequence of certain attractions or properties which the ice and caloric are reſpectively poſſeſſed of. How theſe ſubſtances have ſuch attractions, we are far from pretending to explain. But it is conceived that this theory ought no more to be required to explain the cauſe of attraction, than other theories, on this, and various chemical ſubjects, none of which afford any explanation of ſuch difficulties. There is one circumſtance in this theory which is not quite eaſily conceived, though of the utmoſt importance to the right underſtanding of the doctrine. This is, that water at 32° contains a quantity of heat meaſured by the number of degrees of its temperature, from

the natural zero, multiplied by the number expreſſing its ſpecific caloric. This muſt be very obvious to any perſon who attends to the terms and conditions of the hypotheſis with due care. Yet I have been ſurpriſed to obſerve how many errors inattention to this has cauſed: how many arguments have been ſtated, by men truly ingenious, in oppoſition to this doctrine, which have appeared wholly ill-grounded, or entirely unintelligible, without a previous knowledge of this miſconception.

In this way of conſidering the matter, both parties agree, that in a general groſs view of the ſubject, caloric is the cauſe of fluidity, or rather affinity by which caloric enters into the ſolid. The queſtion is, how does it affect this fluidity? Does it combine with the ſolid in no definite or meaſurable quantity? Or does it enter into it, in a quantity proportionable to the new capacity? And, after all diſcuſſions and diſputes regarding this ſubject, it will, I think, be found that is the real ſtate of the inquiry, in the caſe, at leaſt, where the theory is ſtated as I have now done.

The objection propoſed by Dr. Black, on the ſuppoſition, that in defence of this theory, it might be ſaid, that the ſenſible heat is firſt increaſed a little, ſo as ſuddenly to weaken greatly the coheſive attraction, after which the latent heat enters, does not by any means appear very forcible. Surely no perſon could make ſuch a ſuppoſition, who knew that ice is not expanded, but contracted by fuſion.

From which it ought to follow, that the cohesive attraction, far from being weakened, acts more powerfully at the diminished distances, if at least heat were the cause of fluidity solely by its expanding powers. It cannot be maintained, therefore, that caloric is the cause of fluidity, by separating the particles of the solid, to such a distance from each other as to be out of the reach of each other's attraction. But if it were so argued, the cooling of water to 22° would afford no additional proof. For, undoubtedly, the lower the temperature of water is reduced, the less chance can cohesive attraction have of causing solidity, since every new degree of cooling is accompanied with a corresponding expansion of the water, according to the best experiments. No doubt, it is a very curious fact, that water can be cooled to a lower degree than 32°, and still more extraordinary, that the slightest motion immediately is followed by a crystallization. And Dr. Black's question, what hinders the cohesive attraction from producing its effect, might be varied so as to require an answer from the supporters of his own theory. It might be asked, how does it happen that a chemical combination of ice and caloric, which cannot be decomposed, by the confessedly superior attraction for heat of the cold bodies surrounding, can be suddenly, and as it were magically, dissolved by mechanical means, by a slight percussion? It is clear, that the cold substances around do attract caloric with greater force than water does at other times.

For water, ſurrounded by bodies of the temperature of 22°, will, without the greateſt care to prevent motion certainly be frozen. And it will be found impoſſible to melt ice enveloped in ſubſtances at 22°, by any means. From which it follows, if a chemical union really does take place in this caſe, that there is here an anomaly in the laws of attraction, which is not eaſily reconciled with known principles.

The nature and laws of corpuſcular affinity are certainly ſtill very imperfectly underſtood. And there may probably be varieties, in its mode of action, yet to be diſcovered, ſufficient to explain this and many other chemical difficulties. There are various powers, the extent of whoſe influence is yet almoſt unknown, and which yet probably interfere in many proceſſes of chemiſtry. There may alſo probably be other powers, or ſubſtances, and ſituations, in which already known powers operate. Magnetiſm, though of old, is not of ancient diſcovery: electricity is but recently diſcovered; and Galvaniſm, even if only a new inſtance of the electrical fluid, is a proof of the poſſibility of many agents exiſting unknown, or in ſituations altogether unexpected. Something, therefore, ought to be allowed for ſuch circumſtances, when we reaſon concerning the action of particles among each other. And Dr. Black's queſtion, juſt alluded to, may be anſwered differently on theſe grounds. It may be ſaid, that when fluidity takes place, not only an

abſorption of caloric is obſervable, but the ſpecific caloric becomes greater. From arguments already given it appears, that this latter effect is adequate to the production of the former, and that the new heat muſt enter into the body in a ratio guided by the increaſe of the ſpecific caloric. The fluidity itſelf, however, remains to be accounted for, and if it even were granted, which it is not, that heat according to our theory could not be this cauſe, it would be unwarrantable to ſay that no cauſe could exiſt, while it is recollected how very imperfectly the laws of particular attraction are underſtood, how little we have aſcertained to what degree the influence of electricity is exerted during fuſion, or what other powers happier diſcoveries may yet demonſtrate to operate in producing this effect. For it ſtill remains clear that it is impoſſible to deny that the change of the capacity of the ice muſt, independently of all other circumſtances, be accompanied by an abſorption of caloric by the water formed. Theſe arguments, however, only apply to Dr. Black's ſtatement; and it is again to be recollected that none of theſe ſuppoſitions is at all neceſſary to the truth of the theory in queſtion.

It is aſſerted that as water above 32° is always fluid, ſo water below 32° ought always to be ſolid; but that this is not ſo. The reaſon of this is ſaid to depend on the expanſive power of heat. But ſurely if a ſlight increaſe of ſenſible heat *were* to cauſe fuſion by ſeparating the particles, the cooling of

water below 32° need not produce the oppoſite effect. Ice we know expands by heat, but water as often expands by cold. And if ice at 32° by a ſmall increaſe of ſenſible heat *has* the coheſive attraction of its particles deſtroyed or greatly diminiſhed, it does not by any means follow that water cooling below 32° ought to have the coheſive attraction of its particles increaſed. The caſes are directly oppoſite; and as ice ought to become fluid above 32° by the ſeparation of its particles, ſo water ought to become more liquid every degree that its temperature falls below 40°, for there lies the maximum of the contraction of water.

It is not, however, to be conſidered as a fair ſtatement of the doctrine of capacities, to ſay that bodies firſt change their ſpecific heat, and then admit or abſorb the latent caloric, the ſubſtance being thus left to become fluid in the beſt way it could. In a general view it may be allowable to grant that heat is the cauſe of fluidity in the ſame ſenſe as we ſay that an alkali is the cauſe of an oil becoming ſoap. We ſee, for example, very clearly that ice cannot bear a temperature higher than 32° without fuſion; neither can it be fuſed without heat unleſs by the help of chemical agents. Caloric, therefore, is certainly requiſite for the formation of water. But let us ſuppoſe a quantity of caloric to enter a portion of ice at 32°; if there is enough of caloric, the whole ice is converted into water; if there is leſs than enough, the ice is only converted in part into the fluid form.

The queſtion here is not whether heat is the cauſe of fluidity, but a more difficult one, how does heat produce it. The theory of capacities aſſerts that the caloric ruſhes into the ice enlarging and ſatisfying its capacity at one and the ſame inſtant and by one and the ſame action. The fuſion, the increaſe of capacity, and the heating of the body according to the new capacity are obviouſly ſimultaneous, the work of the ſame moment. It may be difficult to explain how all this happens; but not more ſo than to explain according to any hypotheſis how fluidity ſhould be produced by heat.

The increaſe of the capacity of all ſolids when fuſed, and of all fluids when vaporiſed, is ſurely a ſtriking fact, which deſerves conſideration. Yet it has met with very little or indeed with none at all from Dr. Black's explanation. Not only does ice require a quantity of caloric to exiſt as water, but after it is become water, heat has no longer the ſame effect on it, but raiſes its temperature much leſs; nay, this continues true below 32° Fahr. and as far as we know to the bottom of the thermometric ſcale. We ſee in all caſes, that whenever the capacity is enlarged either of a ſimple body or the mean capacity of two bodies combined, that heat or rather temperature diſappears.

This is without a ſingle exception, that is known, in the whole hiſtory of chemical unions. It is not eaſy to conceive how ice could poſſibly have its power of containing heat increaſed from eight or nine to

ten, without abſorbing heat. It is actually proved that it does increaſe its capacity, and this fact demands explanation and attention as much as the production of fluidity itſelf. We cannot avoid aſking, how can a body augment its capacity without either becoming colder or abſorbing heat. And if this be found difficult or impoſſible to explain, it muſt neceſſarily be admitted that the increaſed capacity is the cauſe of the influx of heat; or, at leaſt, that the increaſe of capacity and abſorption of heat are coexiſtent and ſomehow connected. It ſeems indeed to me altogether undeniable that, at all events, ſome part of the caloric of fluidity, as it is called, muſt go to raiſe the temperature of the body fuſed.

In the caſe of vapour, the ſame general arguments may be applied. If we ſuppoſe the water to be heated to 212°, and caloric ſtill to be flowing in, the caſe of vapolization appears completely ſimilar to that of fuſion. Only in the former inſtance there is no peculiarity in the action of heat with regard to expanſion. Steam at 212° is greatly more bulky than the water from which it was formed. But if we imagine the temperature of the water to be 212°, and no more caloric to be entering, then let the preſſure of the atmoſphere be ſuddenly removed, and a quantity of vapour will be inſtantly produced, and the temperature of the whole will ſink conſiderably. In this caſe it has been ſaid that the removal of the preſſure was the cauſe firſt of the increaſe of the ca-

pacity, and afterwards of the cold produced. But theſe circumſtances occur at one inſtant, and no certain inference of this kind can be drawn. The phenomenon, however, is not to be forgotten in the conſideration of the cauſe of fuſion. One remark further will be admitted, that if the chemical combination of heat be at all allowed, the caloric is as much chemically united with the water at 212°, as it is with the vapour, formed by removing the preſſure; and the ſame is true to whatever degree the temperature of the water be raiſed. Therefore we cannot allow, with the ingenious Dr. Robinſon, that preſſure has the power of preventing chemical combination in this inſtance.

According to this theory, therefore, it is held that caloric, exiſts in all bodies in the ſame ſtate, it matters not whether retained by chemical or other powers. This view is remarkable for its ſimplicity. All bodies are affected in a ſimilar way by heat. Ice and iron, for example, we know to have different capacities for heat. We ſee that each thermometrical degree in each is raiſed by a quantity of caloric proportional to its capacity. This is true as far up and as low down in the ſcale as it has been poſſible to obſerve. We infer that it is true alſo to the point of total privation of all heat. Even, if it ſhould not be ſo, I ſhall afterwards endeavour to ſhew that the argument is unaffected. Iron and ice are therefore concluded to contain heat in proportion to their capacities. But the ſame is true of any other bodies

choſen. The like circumſtances appear alſo in ice and water; their capacities are different, that of water being higheſt. It ought therefore to contain more caloric than ice. But by experiment it actually does. Hence ice and water are to be conſidered in the ſame comparative view with regard to heat, as ice and iron, or glaſs and gold, or any other two bodies having different capacities for heat.

This doctrine ſuppoſes nothing of the intimate nature of heat. It agrees equally well with the hypotheſis of matter or that of motion. The other mode of explanation, however, ſeems to require the previous admiſſion of the material nature of caloric, and that it is governed by the uſual laws of other matter; a ſuppoſition which many philoſophers of the preſent day are by no means diſpoſed to admit.

The advantage of this hypotheſis is, that it affords an explanation of all the phenomena of fuſion; but the contrary theory, though applicable to other circumſtances, does not explain why a body combining with latent heat, ſhould have its ſpecific caloric ever after greatly enlarged. It might as well, for any reaſon aſſigned, happen in directly the reverſe manner; eſpecially when we conſider, that, ſo far from there being more room between the particles of the fluid than between thoſe of its correſponding ſolid, there is plainly leſs in many inſtances Either therefore, the increaſed capacity and the fluidity are the co-exiſtent conſequences of ſome common cauſe,

or the union between the latent caloric and the ſolid is accompanied, at the moment of its formation, with a new capacity for heat in proportion to which the latent caloric is great or ſmall. It is in the preſent ſtate of knowledge no eaſy taſk to ſay, whether any adequate cauſe, beſides caloric, for the production of fluidity is known, or is likely to be diſcovered; it would be preſumptuous to affirm or deny too boldly. I have already alluded to the agency of the electrical fluid, and there can be no doubt that it is ſomehow implicated in the change from the ſolid to the fluid ſtate. Ice is well known to be a non-electric. After fuſion, however, it is one of the beſt electrics known, with the exception of the metallic bodies. I do not know whether it has occurred to any other perſon to try if this property extends any farther. But as it might ſtrengthen the arguments of one or other ſide, I have thought it an object of curious inquiry, and made a few experiments on this ſubject. I cannot ſay, however, that much light has been thrown upon the object of conſideration by theſe trials.

The ſubſtances which I thought of examining were four of the non-electrics moſt eaſily melted. I mean ſpermaceti, bees' wax, common roſin, and ſulphur. A metal cup filled with a ſolid maſs of each of theſe was put upon the conductor of a powerful electrical machine, and ſparks were attempted to be drawn through them without ſucceſs. Only when the coat

of the non-electric was very thin, a few ſparks forced their way through the ſpermaceti and the roſin. Each of theſe ſubſtances was ſubmitted to the ſame trial, but in a fluid form. In the melted wax and ſpermaceti, when a brass rod was approached to them perpendicularly, a violent internal motion took place, ſufficient to drive great part of the materials out of the veſſel. Some ſparks alſo appeared to proceed from the fluid, but a great number came from the bottom of the veſſel, the electrical fluid forcing the melted wax and ſpermaceti out of its place, and leaving part of the bottom of the veſſel uncovered. It appears thus that electricity has a much greater tendency to diſplace theſe two bodies than to paſs directly through their ſubſtance. But I am diſpoſed to think that the wax and ſpermaceti are not ſo complete non-conductors in their fluid as in their ſolid ſtate. The melted ſulphur and roſin alſo gave indications of the paſſage of a ſmall portion of electricity through them more than they did in their ſtate of ſolidity. Upon the whole, though no very poſitive inferences can be drawn from theſe experiments, yet I think that they rather tend to favor the opinion that fluids conduct electricity better than their relative ſolids. It might be worth while to purſue this ſubject farther, and to determine the changes ſuſtained during fuſion, by various ſubſtances with regard to their electrical properties, eſpecially by the more eaſily fuſed metals, ſuch as

mercury, bismuth, lead, tin, and mixtures of these; some of which are very fusible and well fitted for such an examination.

But to return to our more peculiar subject.—I will only very briefly at present allude to the appearance and disappearance of temperature in other cases besides fusion. When various substances are mixed, if a chemical action takes place between them, there is a change in the common temperature. Sulphuric acid and water, for example, mixed in equal quantities, have their temperature very much raised, and at the same time their mean capacity for heat diminished. It may be, and has been said, that the acid and water have a greater attraction for each other than either has for caloric, and, therefore, that caloric is given out or precipitated. Notwithstanding this the heat remains combined with the body, till taken from it by the attraction of the surrounding colder matter. It has been asserted wherever heat appears in this manner, that the two substances occupy less bulk together than they did separately; whence caloric is forced or squeezed out. That is to say, a body, when its particles are very close to each other, ought to contain less caloric than the same body, when its particles are farther separated. But does this hold universally? It can scarcely be answered in the affirmative when it is recollected that, in the freezing of various fluids, a large portion of caloric is elicited, and a considerable

change of capacity takes place; but inſtead of theſe circumſtances being attended with a compreſſion or approximation of particles, an expanſion of the body, that is, a ſeparation of its particles, is obſerved. Beſides which, theſe reaſonings give no explanation of the change of ſpecific caloric. It can only be ſaid it is the fact. But it is a very troubleſome one, and one ſufficient to account for the production or diſappearance of a great quantity of heat. According to our doctrine, any body having its capacity leſſened, muſt neceſſarily become ſenſibly hotter. But here is a body becoming hotter and having its capacity diminiſhed, apparently at once. It is impoſſible to aſcertain, otherwiſe than by inductive evidence, which happens firſt. But it is clear, that mere abſtraction of heat does not leſſen the capacity, but that to explain this, ſome peculiar affection of the bodies with reſpect to caloric muſt be ſuppoſed.

Hence it may be obſerved, that ſince in every inſtance without a known exception, the production or elicitation of heat by the mixing of bodies is accompanied by a diminution of the ſum of the capacities of the bodies, and that every caſe of cold produced is accompanied with an increaſe of the ſum of the capacities, we may infer, that there is a connection probably of cauſe and effect, between theſe phenomena. And ſince the cold or heat appearing do not explain the change of capacity, but this change readily explains the alteration of temperature, we

may be permitted to ſuſpect the change of capacity as the cauſe of the other change. It may, alſo be remarked that a theory, which explains all the phenomena excepting this augmentation or diminution of capacity, cannot be conſidered as equally complete with one which extends to this point, as well as to all others, which are required to be explained.

ESSAY III.

ON THE CAPACITIES OF BODIES FOR HEAT.

The comparative elevations of temperature produced in different bodies by the ſame or ſimilar quantities of caloric, are ſuppoſed to depend on a peculiar property of theſe ſubſtances, which has been diſtinguiſhed by various names. It has been ſtyled by different philoſophers, the capacity for heat, the ſpecific heat, the relative and the comparative heat of bodies. Theſe capacities are regular, that is, each degree of temperature is raiſed in the ſame body by a like portion of caloric: and in many inſtances this has been experimentally proved to be true, through a long range of thermometric degrees, provided no change of form takes place. And in general it may be affirmed, that no exception to this regularity or permanence of capacities has been obſerved in any caſe whatever, excepting

in the inſtance juſt alluded to, when a change of the ſubſtance took place from a ſolid to a fluid, or from a fluid to an aëriform appearance. To compare this property of bodies we may take either equal bulks or equal weights of each. In the caſe of employing equal weights, indeed, it has been objected that the phraſe capacity for heat is only intelligible when equal bulks are taken. This objection does not ſtrike me as very forcible, and it would appear that the capacity of equal weights of two ſubſtances may be compared with as much eaſe and preciſion as the capacity of equal bulks, and be equally well underſtood. For it is by no means neceſſary to reſtrict the meaning of capacity to mere capaciouſneſs. In common language it is at leaſt as often uſed to expreſs the power of acquiring or admitting as that of containing. From the other words uſed to expreſs the ſame idea by Dr. Black and Dr. Irvine as mentioned by Dr. Robinſon, ſuch as affinity for heat, and appetite for heat, it is probable that the meaning of capacity was not intended to be capaciouſneſs alone.

There has been a good deal of diſcuſſion regarding the propriety of uſing the various expreſſions for the property of which we now treat. The word capacity, in particular, has been objected to, chiefly on the ground of its being a theoretical phraſe which ſuppoſes a peculiar ſtate of the caloric in bodies; but, unleſs taken in the ſenſe of capaciouſneſs, it cannot lead to any other inference than all the other words do. Upon the whole, it ſeems to me at leaſt,

that any of the abovementioned modes of expreſſion may be employed without objection, a clear definition of the meaning attached to it being premiſed. The word capacity was firſt employed in this ſenſe by Dr. Black and Dr. Irvine; the expreſſion, relative heat, by Dr. Irvine, comparative heat by Dr. Crawford, and ſpecific heat, I believe, by Profeſſor Wilcke.

The cauſe of the various capacities of bodies for heat does not ſeem to be ſatisfactorily accounted for. The capacity does not follow the ratio of the ſpecific gravity or of any other property of bodies as far as can be determined. It appears, that every body has a peculiar power of attracting or admitting a quantity of caloric, which is as peculiar to itſelf as its weight or its chemical qualities. It would certainly tend to ſimplify the theory of heat very much if any account could be given of this property of bodies, which could be referred to a more general view of the ſubject.

It has been ſuggeſted by the author of an ingenious ſyſtem of chemiſtry that bodies unite with caloric from an affinity which they have for it in like manner as all chemical compounds are formed. But the affinity varying in degree, it follows that different bodies ſhould require different proportions of caloric to form a new compound. Bodies, however, conduct calorie with various celerity. The ſame author attributes the retardation of caloric in its paſſage through theſe bodies to the affinity exerciſed

by the particles of matter near which the caloric paſſes. From which he concludes that the conducting power of bodies is inverſely as their affinity for caloric, or inverſely as their capacity for heat. It follows from theſe ſtatements that the truth of this theory muſt reſt upon the correſpondence of the capacities and conducting powers of bodies as determined by actual experiment. Dr. Thomſon has accordingly made a few experiments with this view, and he found the number expreſſing the conducting powers to be more nearly, as he himſelf ſays, the inverſe of the ſpecific caloric of the bodies as determined in his table than he could have expected in experiments of ſo delicate a nature. His numbers are as follow:

Bodies.	Specific Caloric.	Conducting Power.		Difference.
		By theory.	By experiment.	
Water	1	1	1	0
Mercury	0.31	3.22	4.600	+1.38
Linseed oil	0.528	1.89	1.085	—0.81

If this table were implicitly to be adopted, it would, as far as it goes, be in ſome degree conſonant with the principles of the hypotheſis, excepting that the computed conducting power of mercury differs too much from the experimental concluſion, being conſiderably more than a fourth part leſs than it ſhould be. The differences, too, are not upon

the ſame ſide in both caſes; for if the conducting power of the mercury be one-fourth too ſmall, that of the linſeed oil is too great by more than a third. It would have been deſirable, that Dr. Thomſon ſhould have given a more detailed account of experiments from which he draws ſo very important inferences. There are many minute particulars which tend very much to illuſtrate the application of experiments to theories, of which no account can be taken in a table containing merely a numerical reſult. The ſpecific caloric of mercury is ſtated conſiderably higher than it is uſual to allow it. This concluſion is drawn, as we are informed from Dr. Thomſon's own experiments, and is, as he affirms, nearer the reſults obtained by others than Dr. Crawford's determination is. As upon the accuracy of the numbers in the above ſtated table all the proof of the theory in queſtion reſts, it is proper to inquire into the reaſons of admitting or rejecting the new number for the ſpecific caloric of mercury.

It is certainly juſt to allow the fairneſs of Dr. Thomſon's ſtatement of his own experiments, though he has not deſcended to particulars. But I cannot underſtand upon what principles he has compared his own reſults with thoſe of others in the caſe of the ſpecific caloric of mercury, ſo as to induce him to ſay that his numbers at all approach thoſe of any other philoſopher. In the table of ſpecific heats in that gentleman's ſyſtem of chemiſtry, he gives 0.31

as the mean determination of Lavoiſier and Kirwan. The experiments of the latter of theſe philoſophers, are contained in Magellan's Treatiſe on Heat, which I have not been able to conſult. But in the *Annales de Chimie*, vol. V. where Lavoiſier's experiments on this ſubject are quoted, his number is given as 0.029 which is very wide indeed of Dr. Thomſon's, and would require Mr. Kirwan's to have been no leſs than 0.591 which is extremely improbable. There muſt have been ſome miſtake here. Dr. Black* informs us that the ſame quantity of heat which raiſes the temperature of water two degrees raiſes that of mercury three; therefore, the capacities of theſe two ſubſtances are to each other as 3 : 2. Or water being taken as 1., mercury will be .66. But theſe are the ſpecific heats of equal bulks, and when equal weights are taken, as muſt be done to compare the reſults with Dr. Thomſon's, the proportion ſtands as 1. : : 0.049 nearly. For ſince equal meaſures of water and mercury are heated by equal quantities of caloric as 2 : 3, take the ſpecific gravity of mercury at 13½ in a rough way; the 27 half ounces of mercury gained 3°, while two half ounces of water loſt 2°. But two half ounces of mercury would have gained more in the ratio of 27 : 2, or 40°.5. Therefore the ſpecific caloric of water is to that of mercury in the inverſe ratio of their augmentations of temperature, as 40.5 : : 2, or as 1 : 049. This is certainly ſomewhat different from Lavoiſier's determination, but it is out of all ſight of Dr. Thomſon's.

* Black's Lectures, vol. 1, page 80.

Dr. Crawford* obſerves that the quantity of heat which raiſes a pound of water one degree, raiſes a pound of mercury 28°; or that their ſpecific heats are as 1 : 0.035, equal weights being taken. Dr. Irvine expreſſes the very ſame opinion nearly in the ſame words, and adds that he has repeated the experiment many times. Dr. Crawford has in another part of his work † mentioned that the changes of temperature in theſe two ſubſtances are to each other as 3 : 2, in the ſame terms as ſtated by Dr. Black. There ſeems ſome inconſiſtency in theſe two accounts of the ſame facts.

It appears then, that the experiments of other philoſophers have more nearly coincided with each other than with Dr. Thomſon's. We have by Lavoiſier, by Black, and by Crawford, or Irvine, three numbers given, as below:

Black	0.049
Crawford	0.035
Lavoiſier	0.029
Mean	0.037

The mean here is extremely wide of the number in the former table, and it is impoſſible under theſe circumſtances to give aſſent to deductions of the cauſe of the conducting power of bodies founded

* Eſſay on Animal Heat, p. 5.
† Page 88, ibid.

this nature. Particular care muſt be taken that no chemical union take place among the ſubſtances mixed for examination. Proper allowances muſt be made for the heat or cold communicated by the veſſel employed, and for the change of temperature produced by the ſurrounding air. When theſe circumſtances are attended to, large quantities of the materials being uſed and at various temperatures, the reſults will probably approach to accuracy. Perfect accuracy can never be attained, but care and ſkill will remove or leſſen almoſt every obſtruction. Water has been generally uſed as the medium of compariſon, its own ſpecific heat being eſtimated at unity. Where no chemical action is exerted by water on the ſubſtance, of which we wiſh to aſcertain the capacity, it is frequently convenient to uſe it as one ingredient of the mixtures. In general there are three different ways of mixing bodies with each other for this purpoſe: 1, Equal bulks or equal weights may be taken ſo as to produce unequal changes of temperature, and then the capacities are inverſely as the changes of temperature produced; or 2dly, Unequal quantities may be mixed ſo as to produce equal changes of temperature, in which caſe the capacities are inverſely as the quantities of matter; or, 3dly, Unequal quantities may produce unequal changes of temperature, and then Dr. Irvine ſhewed that the capacities are reciprocally as the quantities of matter multiplied into their reſpective changes of temperature. In all theſe ways a great degree of accuracy may be attained, and I can

ſee no foundation for the opinion that equal bulks or weights are preferable to unequal ones; on the contrary, the eaſieſt and moſt certain way ſeems to be the third. It is frequently neceſſary to uſe a third ſubſtance as a medium with which two others may be compared, that their relative capacities may be diſcovered. This is requiſite when two bodies have a chemical action upon each other, as ſulphuric acid and water. It would be impoſſible to diſcover the ſpecific heat of that acid by mixing it with water in any proportions. A third body muſt be mixed with each: and in this inſtance, as will be ſeen in the ſecond part of theſe Eſſays, Dr. Irvine employed pounded glaſs, adding to a given weight of each a quantity of glaſs ſufficient to change their temperature by the ſame number of degrees: then the capacities were as the weights of glaſs added. This method differs from all thoſe already ſtated, and, in general, when equal quantities of two different bodies are examined by help of equal quantities of a third, the capacities are inverſely as the changes of temperature, but if equal changes of temperature be produced by unequal quantities of the third body, then the capacities are directly as the maſſes added. But if unequal weights or meaſures be taken, the capacities may eaſily be found with reference ſeparately to the third body in one of the three methods firſt ſtated.

Proceeding in one or other of theſe ways, Dr. Irvine determined the capacities of a multitude of bodies, chiefly with a view to certain opinions re-

garding the nature of caloric which he held or wiſhed to examine. I am poſſeſſed, however, of the reſults of comparatively few of his experiments. But I have ſubjoined his numbers expreſſing the capacities of ſuch bodies as I have been able to aſcertain to have been inveſtigated by him, in the following table:

Substance.	Capacity of equal weights.
Water	1.000
Ice	0.800+
Oil of Turpentine	0.400
Iron	0.143
Crown glass	0.200
Spirit of wine	0.930
Fluid Spermaceti	0.320
Sulphuric acid	0.333

It has been obſerved above, that bodies in general have their ſpecific caloric increaſed during their fuſion. For the moſt part, this change is performed at one thermometrical degree and at the ſame moment. But this is not without exception; for ſome bodies paſs from the ſolid to the fluid form not directly, but after undergoing various degrees of ſoftneſs which at laſt terminates in perfect fuſion. Of this kind we may enumerate bees' wax, ſealing-wax, ſpermaceti, iron, and probably ſome others. Dr. Irvine made ſome experiments upon this ſubject by obſerving the rate of cooling of a maſs of fluid ſpermaceti or bees' wax. The reſult of theſe is as follows:

EXPERIMENT I.

On the cooling of melted Spermaceti.

Hour.	Degree of Heat.	Diff. of Degrees.
12. 10′	128	
11	123	5
12½	120	3
14	118	2
16	118	0
18	118	0
20	118	0
22	117	1
24	116½	½
26	116	½
28	115	1
30	114½	½
32	114	½
33	113	1
34	112½	½
35	112	½
37	111	1
38	110	1
42½	109	1

EXPERIMENT II.

Hour.	Degree of heat.	Diff. of Degrees.
1. 18′	156	
20	140	16
22	128½	11½
24	122½	6
26	119½	3
28	118	1½
30	118	0
32	118	0
34	117½	½
36	117	½
38	116½	½
40	116	½
42	115	1
44	114+	1—
46	113½	½
2. 52	71	42½

EXPERIMENT III.

Hour.	Degree of Heat.	Diff. of Degrees.
3, 7′	160	
9	143	17
11	131	12
13	123	8
15	119½	3½

In this experiment the firſt congelation began when the temperature was 127°.

EXPERIMENT IV.

Hour.	Degree of Heat.	Diff. of Degrees.
3. 21	157	
23	142	15
25	131	11
27	123	8
29	119½	3½
31	118	½
48	118	0

In this experiment the thermometer was kept in the centre of the fluid. The firſt congelation began at 126° +, and all was ſolid, though ſoft at the laſt obſervation.

In all theſe experiments but the fourth, the thermometer was kept at the edge of the fluid. The temperature of the air was 55°. The quantity of ſpermaceti uſed was one ounce.

EXPERIMENT

On the Cooling of Melted Bees' Wax.

Hour.	Degrees of heat.	Diff. of degrees.
12.54′	176	
56	154	22
58	142	12
1. 0	142	0
2	142	0
4	140½	1½
6	130½	1
8	138	1½
10	136½	1½
12	134½	2
14	133	1½
16	131	2
18	129	2
20	127	2
22	125	2
24	123	2
26	120½	2½
28	118	2½
30	116	2 +
32	113½	2½ —
34	111	2½
36	109	2
38	107	2
40	105	2
42	103 —	2 +
44	100½	2½
46	98	2½
48	96 —	2½
50	93½	2½ —
52	92	1½
54	90½	1½
56	89	1½
58	87	2
2. 0	85	2
2	83½	1
4	82½	1
6	81½	1
8	80	1½
10	78½	1½
3. R.	57	21½

In this experiment the thermometer was kept at the edge of the fluid. The quantity of wax amounted to one ounce and a drachm. The temperature of the air was $52\frac{1}{2}$.

It was from theſe or ſimilar experiments, that it was concluded by Dr. Black and Dr. Irvine, that theſe bodies, which ſoften previous to their complete fuſion, take in a part of their latent heat during their ſoftening, and give it out again during their gradual hardening. Dr. Irvine conceived further, in purſuance of the principles of his theory, that theſe ſubſtances change their capacities for heat gradually. It ought to be obſerved that this forms no exception to the principle that bodies only change their capacity with their form. For we have here a gradual and progreſſive change of a capacity, at every ſtep of which a correſponding change of form takes place. It is in vain to bring this as an argument againſt the permanence of capacities. With as much reaſon it might be objected that ſolids in general change their capacities during fuſion, and fluids when they aſſume the gaſeous form. There is no change of capacity during the ſoftening of bodies of which warning is not given to the ſenſes by an alteration in the appearance of the body approaching to fluidity; and the advocates of the oppoſite opinion may be challenged to produce any inſtance of an alteration in the ſpecific caloric of any body which does not accompany a more or leſs conſiderable change of form.

The knowledge of the capacities of different ſub-

ſtances is of importance in various points of view, but eſpecially with regard to the quantity of caloric contained in bodies. It is impoſſible to determine to what uſes the tables of ſpecific caloric may be applied in future, but a ſtore of facts always turns to advantage at one time or other. It is probable that if ever philoſophers ſhall arrive at a more diſtinct knowledge of the actions of the ſmall particles of matter upon each other, an accurate acquaintance with the ſpecific calorics will be of much importance. In the mean time our preſent conſideration muſt chiefly be directed to capacities as the probable meaſures of the abſolute heat of bodies.

It is to be inquired then, do bodies contain caloric in proportion to their ſpecific heats. This appears to be highly probable, when it is conſidered that the experiments for determining the ſpecific heats of bodies are only obſervations of the changes of temperature, produced in equal quantities of each by equal quantities of heat, and that it appears, that as far up and as low down as theſe experiments have been made in the thermometric ſcale, there is no alteration of capacity, while the body retains the ſame form. For the perfect juſtneſs of this obſervation it is only neceſſary to refer to the experiments of the beſt authors on this ſubject, as for example, Crawford and Wilcke. As far then as actual obſervation has gone, capacities are permanent, and it is reaſonably inferred that the ſame thing is true to the point of total privation, ſince of the contrary ſuppoſition there is neither experimental nor theoretical evidence. If it

were poſſible to add experimental proof of this, it ought undoubtedly to be done. But, this clearly is and always muſt be impoſſible. Since then there is not the ſmalleſt aſſignable reaſon why the capacities of bodies ſhould vary in extreme colds, but exceedingly plauſible arguments why they ſhould be permanent; let us for a moment conſider what conſequences ought to follow the one or other of theſe ſuppoſitions, and compare them with the actual ſtate of facts. It is evident, that if capacities are permanent, two bodies which diminiſh their capacities when mixed, ought always to become warmer than they were before mixture. And this is by experiment found to be the caſe without a ſingle exception. On the other hand, bodies which become warmer by mixture ought, if capacities are permanent, or, in other words, if bodies contain caloric according to their capacities, always to have the capacity of the mixture leſs than the mean of the capacities before mixture. And this is no leſs certainly and univerſally true than the former. Theſe coincidences are ſurely very remarkable, and ought to make us pauſe to conſider whether this accompanying change of capacity is competent to account for the changes in the ſituation of the caloric of the bodies. But ſince it is competent to this purpoſe, it is unneceſſary to make another ſuppoſition to account for the phenomena. For no theory can poſſibly deny that the change of capacity is more or leſs concerned with the changes of temperature. And it is contrary to the maxims of ſound philoſophy to

make more ſuppoſitions than are ſufficient to explain the facts.

We are acquainted with three ſtates in which moſt, if not all bodies may exiſt with regard to heat, the ſolid, the fluid, and the aëriform ſtate. It has been ſuppoſed, and is not wholly impoſſible, that there may be ſome other ſtate at very low temperatures. It may alſo be ſuppoſed, for it cannot be demonſtrated, that capacities may vary in extreme colds. Of the firſt of theſe ſuppoſitions it is needleſs here to ſay any thing, as it is equally out of our power to prove and diſprove ſuch an hypotheſis, and allowing it to be true it would affect the reaſoning concerning the abſolute heat of bodies, only in as far as a change of capacity accompanied a change of form. The conſideration therefore, of the effect of a change of capacity on the computation of the quantity of heat contained in a ſubſtance, will include all caſes. It has been generally ſuppoſed that bodies cannot contain heat in proportion to their capacities, unleſs the capacities be permanent. M. Seguin has poſitively aſſerted, that this preliminary is neceſſary to be admitted if this doctrine be true, and the aſſertion has paſſed current. As far as I perceive, however, ſuch a change will make no alteration whatever on the abſolute heat of any body computed before the change of capacity takes place. At leaſt whatever differences may ariſe, no new argument can be derived from that ſource, further than from the already agitated point of diſpute,

the change of capacity during the proceſs of fuſion.

The propoſition to be defended is, that bodies contain heat in proportion to their ſpecific caloric. If the capacity remain permanent to the natural zero or point of total privation of all heat, it appears that an equal portion of caloric will be required to raiſe the temperature of the body each degree to the melting point. Conſequently, if the natural zero be the ſame point in all bodies, any two ſolid ſubſtances at the ſame degree of temperature contain heat proportionally to their capacities. For the number of degrees is the ſame in both, and the whole number of the degrees in any one is equal to the whole in any other, and each degree in both bodies is raiſed by a portion of caloric, proportional to the capacity of each. Whence the whole heat in the one body, is to the whole heat in the other body, as the capacity of the one is to the capacity of the other, if the capacities continue permanent, and all experiments concur to prove this to be the caſe, as far as it has been poſſible to make them.

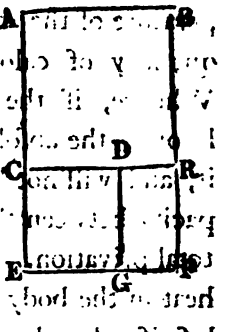
If however the capacity be ſuppoſed to vary, it muſt either diminiſh or increaſe. Firſt let it diminiſh, then the heat of the body in its ſtate of diminiſhed capacity, plus the heat given out during the whole change, is equal to the whole heat of the body before the change. The body in each ſtate contains heat proportionally to its capacity, and the quantity given out is the difference of the abſolute heats in theſe two caſes. Every degree of the tem-

perature of the body in both ſtates is raiſed by a quantity of caloric proportional to the capacity. Whence, if the higher capacity of the body be known, the abſolute heat will be in a known ratio to it, and will not be affected by any diminution of capacity between the melting point and the point of total privation. The object is to aſcertain the whole heat in the body as it actually is, not what it may loſe if reduced to a lower temperature.

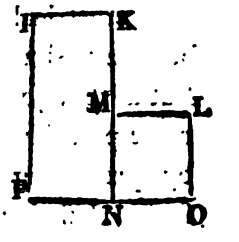
But if the capacity be ſuppoſed to increaſe, the very ſame arguments apply as in the former caſe, and it is of no importance what heat may flow into the body at low degrees of temperature, ſince none of that heat is now in the body, and we wiſh to know the actual ſtate of the ſubſtance with regard to caloric, and not any increments which in certain circumſtances it might receive, but by which it is not at preſent affected.

Though this reaſoning is applied only to one change of capacity, it may be equally extended to any number of alterations, and that whether the changes be all diminutions, all enlargements, or any mixture of both. This is ſufficiently evident without any further demonſtration.

Geometrical figures cannot in caſes of this kind be admitted as proofs on either ſide of the queſtion. But as they ſometimes tend to illuſtrate and render more perſpicuous a ſubject in itſelf rather obſcure, by preſenting the evidence more directly to the ſenſes, I ſhall now repeat the reaſoning above ſtated with the help of a diagram.



Let there be any body whoſe capacity is repreſented by the line AB, and the number of degrees in which from the point of total privation of heat is repreſented by the line AE at right angles to AB. Complete the parallelogram AEFB. It is affirmed that that parallelogram repreſents the whole heat in the body computed from the point of thermometric elevation A to the natural zero E. This is obviously true, if it be allowed, as every experiment renders probable, that the line AB is equal to the line EF, and that if any line be drawn parallel to AB, and cutting the lines AE and BF, the part intercepted by theſe lines is equal to AB. But if this is not granted, then ſuch a line muſt either be leſs or greater than AB. Firſt let it be found by experiment to be leſs, as for example CD. Produce CD to R. Then the abſolute heat of the body at the temperature C is CG, and the abſolute heat at the point A is compoſed of the figures AR + CG + RG, which laſt figure is the heat given out during the ſuppoſed change of capacity. In the figure INL it might be ſhewn in a ſimilar manner, that an increaſe of capacity would make no difference in the abſolute heat of a body computed from a ſuperior temperature to that at which the change



takes place. The point to be determined in this caſe is the amount or value of the figure IPNK, and it is clear that this may be found without any reference to the additional figure MNOL which does not exiſt at the temperature I.

It muſt be underſtood that I offer this as an illuſtration only, or at moſt a different mode of ſtating the argument, and by no means as a mathematical demonſtration, of which the ſubject is not ſuſceptible. Enough however has been ſhewn, if I judge truly, to make it clear that M. Seguin's opinion and that of ſome later authors, who have borrowed it from him, is not well founded, and that whether the doctrine of capacities be true or falſe, this objection at leaſt, has no weight. The objection is entirely theoretical without any experimental ſupport, and my view here has been to ſhew that the theory fairly underſtood is not obliged to preſuppoſe any permanency of the ſpecific heat of bodies.

From theſe arguments, I hope it will appear, that the only point which admits of diſpute is, whether bodies in general give out or take in caloric, during a change of form in ſuch quantity as to make the new abſolute caloric proportional to the new capacity. To determine this by irrefragable evidence, it would be neceſſary previouſly to know the abſolute heat of the body examined, our knowledge of which point will be diſcuſſed in a ſucceeding eſſay on that ſubject. It is clear, that to confute the propoſed doctrine, would require the ſame procedure. But,

as the preciſe amount of the whole heat of bodies cannot yet be ſaid to be accurately aſcertained, we muſt be contented with conſidering the other parts of the argument, and it is from ſuch conſiderations that we have concluded that bodies do really contain heat in proportion to their capacities. It has been remarked, that as far as actual experiment has gone, caloric enters into ſubſtances in a quantity determinable by their ſpecific heats, excepting only when a change of form enſues. It has been ſuppoſed, however, by ſome philoſophers, that as various chemical agents combine with each other only in definite quantities, ſomething of this ſort may take place in the relations of heat with matter, and that the lower degrees of temperature may be raiſed by ſmaller additions of caloric. The illuſtration choſen in this inſtance, that of the union of oxygen with metals and other combuſtible ſubſtances, does not ſeem at all likely to ſtrengthen the argument. Oxygen and many bodies unite with other ſubſtances, only in three or four different doſes or portions. But ſurely this does not bear the remoteſt analogy to the caſe of caloric, which combines with or enters into bodies in any imaginable quantity without a known limit. If an illuſtration muſt be choſen from the combinations of bodies by chemical attraction, let one be taken which ſhall analogiſe in all points. It ſeems more reaſonable to have recourſe, for the ſake of compariſon, to ſuch an union as that of ſulphuric acid with water, which can be effected with any propor-

tions of the two ingredients. And in this inſtance, it does not appear that there is any decreaſing or increaſing ratio of the quantities of the acid neceſſary to form a ſeries of combinations with the water, nor is there any reaſon to ſuppoſe that the original, or firſt unions of the acid with the water, contains leſs or more acid than the ſucceeding ones. Nothing can be more theoretical, than the ſuppoſition of the neceſſity of the new unions of caloric being conſtantly formed, not by the original particle, but by the new compound. Nor can we admit any evidence from ſuch ſources, to ſtand againſt the more ſimple deductions from facts. It does not ſignify to us ſo much, to know how the union is formed, as to learn the ratio in which the ingredients enter.

In conſidering therefore, the effects of a change of capacity upon the temperature of any body, it appears neceſſary in the preſent ſtate of knowledge to allow, that ſuch temperature muſt be increaſed with the diminution and diminiſhed with the increaſe of the capacity; and in all caſes where a change of capacity and a change of temperature occur at the ſame inſtant, that certainly a part, and moſt probably the whole of the heat appearing or diſappearing is to be accounted for on the principles here endeavoured to be eſtabliſhed. The very idea of cauſe and effect is cloſely connected with ſimple conſequence, and has even by ſome ingenious philoſophers been ſuppoſed to be only a general obſervation of the conſtant appearance of two circumſtances

in ſucceſſion to each other. But in all the phenomena of nature, regarding caloric, there is ſcarcely an inſtance where heat is produced or temperature raiſed withqut a correſponding change of capacity. We except alone, the production of heat by the ſun's rays and the caloric collected by friction. Even theſe may, perhaps, be referable to the ſame head when duly examined, and they are but a ſmall part of the objects of diſcuſſion, and do not bear peculiarly upon our argument. But, to what is this almoſt univerſal coincidence of the change of capacity and the change of temperature to be attributed? Ought we not to conclude, that the one of theſe muſt, in ſome way, be the cauſe of the other, and ſince the change of temperature cannot be made to account for the change of capacity, that the change of capacity which affords an adequate explanation of the alteration of temperature, is the cauſe of that alteration.

M. M. Lavoiſier and Laplace have entered into ſome diſcuſſion regarding the manner in which caloric is combined with bodies. They ſuppoſe that a body can contain heat according to its capacity, only in as far as that heat is admitted among the particles of the body, but by no means, if the caloric enter into the molecules themſelves. The diſcuſſion of topics ſo obſcure as the action of particles upon each other, is not often very ſatisfactory, and it appears totally impoſſible to aſcertain whether caloric does or does not enter into the molecules. It ought firſt to be aſcertained what a molecule is to be conſidered to be,

whether an aſſemblage of ſmaller molecules, or an indiviſible particle. In the former caſe, the molecule may be penetrated by caloric according to the ſame laws as larger maſſes are. In the latter, there ſeems no reaſon to allow it to be penetrable at all. Luckily it is of no great importance which of theſe is true, and the ſubject may be more beneficially conſidered without adverting at all to the action of the ultimate particles of matter.

Another gentleman, Dr. Thomſon, of Edinburgh, has objected to the theory of capacities upon other grounds. I had occaſion to take notice of certain opinions of his upon this ſubject in another place.* Theſe opinions were originally inſerted in the ſupplement to the Encyclopædia Britannica. They were a ſecond time brought forward by that gentleman, in his Syſtem of Chemiſtry; I quote his own words.†

"An objection to this explanation of fluidity (Dr. Black's) was ſtarted by Dr. Irvine of Glaſgow. According to him, the phenomena may be explained without having recourſe to the caloric of fluidity at all. For the ſpecific caloric of water being greater than that of ice, a greater quantity of caloric is neceſſary to raiſe water to a given temperature than is neceſſary to raiſe ice to the ſame temperature.

* Nicholſon's Journal, Vol. 5th.

† Thomſon's Syſtem of Chemiſtry, Vol. 1, p. 326, Firſt Edition.

The inſtant, therefore, that ice is converted into water, its temperature would ſink conſiderably if it did not abſorb an additional doſe of caloric. The caloric, therefore, which is abſorbed during the melting of ice does not combine with the ice and convert it into water, but after the ice has been melted it is abſorbed by the water generated. This theory was zealouſly adopted by Dr. Crawford.

"Dr. Black obſerved very juſtly, that it does not account for the production of fluidity at all. The ſpecific caloric of water is, indeed, greater than that of ice; but how is ice converted into water? This is an objection, which the advocates of Dr. Irvine's or Dr. Crawford's theory (as it has been improperly called) will not eaſily anſwer. But independently of this, the theory cannot be admitted, becauſe it proceeds on miſtaken notions reſpecting ſpecific caloric; becauſe it is inconſiſtent with the phenomena which it pretends to explain, and becauſe it leads to abſurd and contradictory conſequences.

"Let us ſuppoſe, according to the experiments of theſe philoſophers themſelves, that the ſpecific caloric of ice is to that of water as 9:10, and that we mix together equal quantities of ice at 32, and water at 172, the temperature after mixture, ſuppoſing the ice not to melt, ought to be 106°; becauſe, for every 9° which the water loſt, the ice would receive 10°. But as the ice melts, its ſpecific caloric will become equal to that of water, that is to ſay, it will abſorb $\frac{1}{10}$ more of caloric than we have ſuppoſed. We

muſt, therefore, diminiſh the heat of the mixture, by one half of this, or 5°; conſequently, according to Dr. Irvine's theory, the temperature of the mixture ought to be about 100°; but this is contrary to fact, for it is only 32°. The theory, therefore, cannot be true. But farther, ſince equal quantities of ice at 32 and water at 172 after mixture, are only of the temperature of 32°, it follows, if Dr. Irvine's theory be true, that the ſpecific caloric of ice is 140, and that of water 0, or at leaſt that the ſpecific calorie of ice is infinitely greater than that of water; for the water is cooled down 140°, and the ice is heated 0°; conſequently the caloric, which is ſufficient to heat water 140, is not ſufficient to produce any ſenſible effect upon ice, or rather melted ice, that is to ſay, water. This conſequence is ſo palpably abſurd and contradictory, that it is ſufficient to overturn any theory from which it follows."

This ſtatement differs in no reſpect, but in greater length and more complete miſrepreſentation of the ſubject, from that of which I endeavoured to ſhew the fallacy in the 5th volume of Mr. Nicholſon's Journal. If I do not miſtake, it would be ſufficient to allow theſe arguments to be their own anſwer. But to remove all doubt, as far as I can, I will now proceed to point out the objectionable parts of Dr. Thomſon's reaſoning.

Dr Thomſon begins by repreſenting the theory in queſtion, as if the ice firſt had its capacity enlarged in the ratio of 9 : 10, and then took in the latent heat, a ſtatement neither neceſſary nor fair, as I

have elſewhere attempted to prove. The change is inſtantaneous in each particle, and whether the increaſe of capacity be the cauſe of the fluidity or the reverſe, or if ſome other and unknown cauſe operate, it is impoſſible to decide. But there is no reaſon for objecting to the theory, that the enlargement of capacity takes place before the entrance of the caloric. They may be conſidered as ſimultaneous. If on the one hand, the cauſe of fuſion is demanded, on the other, ſtands the increaſe of ſpecific heat, as remarkable, and equally in need of explanation.

In the third paragraph, where Dr. Thomſon employs ſo much mathematical reaſoning to divide 140° of caloric, between ice and water, he conceives the ice to begin the enlargement of its ſpecific heat at 32°, though Dr. Irvine, and all the philoſophers who have followed his doctrines, as far as I know, computed this increaſe to begin at the natural zero.

There is little wonder that the numbers did not agree in this way of reckoning. If the change be made from 32° to the natural zero, the matter becomes plain. Calling the point of total privation of all heat 1260° below 32° as meaſured by the capacity of water, or 1400° by that of ice, the capacities of ice and water being as 9 : 10; and the caloric of fluidity being 140° by the capacity of water, or 155°·555 nearly by that of ice, let equal quantities of ice at 32° and water at 172° be mixed, the temperature is 32°, and the whole is become fluid. But where are the 155°·555 loſt by the water? They

are gone to make up the difference between each degree of the ice from 1400° below 32° up to 32° and each degree of the water between the ſame points. That is to ſay, 155.°555 are equal to the difference between 1400°, meaſured by the capacity of water, and 1400°, meaſured by that of ice. Therefore, from 1400°, by the capacity of water, take 155°.555 by that of ice, or what is the ſame thing, 140° by that of water, and there remains 1260° meaſured by the capacity of water, equal to 1400°, meaſured by the capacity of ice. From which it appears, that the numbers agree, and the proportions are in perfect harmony. I may here remark, that it is a great advantage to expreſs the abſolute heat of the ſolid in degrees meaſured by the capacity of the ſolid, and that however accurately the abſolute heats of bodies be determined, the natural zeros can never coincide, unleſs the numbers are governed by the capacity of the body in queſtion, which circumſtance is occaſionally overlooked by authors.

Dr. Thomſon obſerves, that when ice melts, its ſpecific caloric will become equal to that of water; that is, it will abſorb $\frac{1}{10}$ more of caloric than we have ſuppoſed, and in this aſſertion lies his greateſt miſtake. He ſuppoſes the abſorption of caloric in connexion with the new capacity to begin only at 32, and therefore his argument does not apply to the theory of capacities, but to an imaginary hypotheſis of his own. He is further wrong in aſſerting that ice abſorbs $\frac{1}{80}$ more heat on its fuſion,

It abſorbs $\frac{1}{9}$; as for example, if ice contain 1260° at 32, in melting, it takes in 140, which is $\frac{1}{9}$ of 1260°, not $\frac{1}{10}$, and ſo univerſally. Surely if Dr. Thomſon's ſtatement were right, his mixture of ice and water would not have a different temperature from one of two portions of water at 32° and 172°, that is a mean. What is meant by diminiſhing the heat of the mixture by one half of $\frac{1}{10}$ or 5°, I am at a loſs to comprehend.

It can hardly be neceſſary to follow this gentleman through the reſt of his argument. Yet I may aſk, who knows not that experiments for determining ſpecific heats muſt not be made on bodies while they change their capacities. Notwithſtanding which, Dr. Thomſon finiſhes by inferring, that the capacity of ice is greater than that of water, becauſe 140° from water have not heated ice at 32° at all, but only melted it; and this he aſſerts in the very face of a change of ſpecific heat from 9 to 10.

To conclude, this is called a conſequence ſo palpably abſurd and contradictory, that it is ſufficient to overturn any theory from which it flows.

Since writing the foregoing remarks, another edition of Dr. Thomſon's work has appeared, in which, though what I conſider as the principal errors concerning Dr. Irvine's theory are retained, ſome points are a little altered. The ſame gentleman alſo, in his anſwer to the Edinburgh Review of his work, has adverted to my former ſtrictures on his Eſſay on Chemiſtry in the ſupplement to the Encyclope-

dia Britannica. He there obſerves, that he is far enough from thinking that I have eſtabliſhed my father's opinions. I hope I am not ſo thoroughly blinded by ſelf-love, as to imagine even for a moment, that I have been ſo fortunate as to ſucceed in that attempt. But I can with equal truth aſſure my ingenious opponent, that I am juſt as far from thinking that he has confuted them. Which of us, or whether either of us is right, muſt be left to others to decide. I am not inſenſible that many important objections have been propoſed from quarters of great authority to my father's doctrines. It has in theſe Eſſays been my deſire, however inadequate my power, to ſhew the irrelevancy of theſe arguments. Dr. Thomſon has, with peculiar care and conſiderable fidelity, detailed all or the greater part of theſe objections. But I confeſs, perhaps it is my misfortune, that I cannot ſee any force in the additional arguments which he has invented upon this ſubject. Theſe have always appeared to me to proceed upon a miſconception of the theory in diſpute.

The matter of ſcientific works is ſo much more important than the manner, that it is ſcarcely worth while to advert to the latter. Yet Dr. Thomſon could not be ſurpriſed that I ſhould not be altogether gratified with the manner in which he accuſtomed himſelf at firſt to diſcuſs my father's opinions, if he reflected on the air of ſagacious ſuperiority, the appearance of eaſily clearing away difficulties,

which had puzzled and do ſtill perplex the brains of leſs fortunate enquirers, with which that part of his work was compoſed. I am perſuaded Dr. Thomſon did not intend to produce ſuch an effect, and am ready to admit that theſe obſervations do not apply to the laſt edition of his generally able performance.

It is with ſome regret I obſerve, that that gentleman has accuſed me of want of candour, in criticiſing his article in the Encyclopædia Britannica, rather than his ſyſtem of Chemiſtry. This charge it becomes me to repel, and I truſt I ſhall not attempt to do ſo without ſucceſs. The ſtate of the fact was, that it ſo happened, that I was in poſſeſſion of the firſt but not of the laſt of theſe publications, and I was ſatisfied with conſulting the Syſtem to aſcertain that no improvements had been made. I then imagined that I had obſerved that the arguments were nearly, or at any rate, materially the ſame in both, and that of conſequence, it mattered very little which I endeavoured to anſwer. Even yet, upon referring to the works, I can perceive no eſſential difference, excepting that it is no longer aſſerted, that the ſpecific caloric of iron is greater than that of water. As to the other points, I have in this Eſſay already attempted to ſhew, that I might juſt as eaſily have replied to the arguments in the Syſtem of Chemiſtry, as to thoſe in the Encyclopædia. Though, after all, I do not ſee any right a man has to interpoſe the ſhield of a new work, to defend the frailer body of another performance, the imperfections of which, if even he has

abandoned, he has not retracted. At all events, I hope I have ſaid enough to ſhew, that I was not compelled to have recourſe to the moſt miſerable ſubterfuge of attacking an old book, becauſe I was unable to anſwer a newer and more perfect edition.

Dr. Thomſon has withdrawn from the ſecond impreſſion of his Syſtem many of the arguments which have been here ſtated: they are not to be found at leaſt in his chapter upon Specific Caloric. But that he ſtill retains the very ſame views, is obvious from his obſervations upon abſolute heat. I have, therefore, ſuffered my reply to his arguments to retain its place in this Eſſay.

ESSAY IV.

ON THE LOWEST DEGREE OF HEAT.

It is probable, that at firſt ſight, there muſt formerly have appeared little difficulty in aſcertaining the loweſt degree of heat. People muſt naturally have imagined, that when water was frozen it no longer contained heat. But this idea could continue only till the obſervation of the progreſſive fall of the mercurial thermometer, far below the freezing point of water, and at laſt of the actual ſolidification of the quickſilver itſelf. At what point then are bodies wholly deprived of caloric? Boerhaave, at one period, imagined that the natural zero was at the degree of cold produced by a mixture of ſnow and ſalt, which he found to be at the zero of Fahrenheit's ſcale, though there has ſince been produced a greater cold by the ſame mixture. Fahrenheit him-

ſelf, however, by means of nitrous acid and ſnow mixed together, diminiſhed the temperature to 40° below his own zero, and this was boldly aſſerted to be a degree of cold, ſuch that, if it occurred in nature, would deſtroy all vegetable and animal life. It is now well known, however, that mercury freezes at this point, and the ſolidification of that fluid has been repeatedly effected, by the intenſe colds of Siberia and Hudſon's Bay. Almoſt every period of modern philoſophy has fixed a point for the privation of all heat, which ſucceeding obſervations have removed lower down.

For want of knowing the ultimate limits of heat and cold, we are unable to determine the geometrical proportions of the real or abſolute quantity of heat in one body to that in another. All that we know is, their arithmetical difference. Our knowledge of caloric, therefore, is exactly ſimilar to what we have of a chain of which the extremities are hid from us. We can tell, that ſuch a link is three times as far from one as it is from the other: but whether it is at the middle, or a quarter of the length from the one end, we know not. When Sir Iſaac Newton ſays, that red hot iron is three or four times hotter than boiling water, and that boiling water is three times hotter than the temperature of our ſkin, he reckons the loweſt degree of heat to be when water begins to freeze. And if this be granted, the expanſions of the thermometer are in theſe proportions to each other. But the abſolute or real quantities of

heat in theſe bodies do not differ ſo much as by this way of reckoning they have been computed. If we ſuppoſe the loweſt degree of heat to be when air loſes its elaſticity, which Amonton imagined to be, when utterly deprived of heat, and which, according to him, would be at —400, then the heat of boiling water will be to that of the human body as 400+212 to 400+97, or as 6 to 5 nearly. None, before ſuch an examination, would have imagined, that the abſolute heat in boiling water, which deſtroys vegetable and animal life, was to the abſolute heat in the human body, which is perfectly mild and temperate, only as 6 to 5; and we ſhall find reaſon to ſuſpect that their difference is yet ſmaller.

It has been already ſtated in the foregoing eſſays, that Dr. Irvine took a peculiar view of the properties of caloric. The circumſtances of this view I have endeavoured to explain above. In conſequence of his theory, he was led to the invention of a method of inveſtigating the abſolute heat of bodies, or, what is the ſame thing, of aſcertaining the natural zero, or point of total privation of heat. This method was founded upon the conſideration of the change of the capacity of bodies, during their fuſion, and of the quantity of caloric neceſſary to produce fluidity, called by Dr. Black latent heat. I am in this inſtance again enabled to quote Dr. Irvine's own language, for the explanation of this ſubject. This is a continuation of the paſſage already quoted in Eſſay II. page 59.

"The number of degrees by which a body becomes hotter or colder by changing its form, will be proportional to the quantity of heat in it at the time this change happens, ſuppoſing the capacity in a ſolid form to be, to the capacity in a fluid form, the ſame in all bodies. Thus, in the inſtance already given, the quantity of heat x, which applied to the ſolid produced expanſion 20°, in the ſame body, in a fluid ſtate, produced expanſion 10. Here the capacity of the ſolid is to that of the fluid as 10 to 20, or as 1 to 2, the higheſt number belonging to the one heated with the greateſt difficulty. In this caſe, whatever be the ſenſible heat of the body when ſolid, one half of it will diſappear upon its becoming inſtantly fluid, and *vice verſa*. If the abſolute quantity denoted by x produce a degree of ſenſible heat $= 20$ in the ſolid body, in the fluid it will produce but 10. If x produce 200 in the ſolid, in the fluid it will produce 100, &c. Or, in the firſt caſe, a quantity of heat $= 10$ will, according to Dr. Black, have become latent, and have loſt the property of expanding the liquor in the thermometer, in the ſecond 100, in the third 1000, &c. On the contrary, if the quantity of heat x in the fluid produced expanſion 10, upon change of form it will produce 20, if 100, it will produce 200, if 1000, 2000. Or a quantity of heat $= 10$, or 100, or 1000 will change from latent to ſenſible heat, and recover the property of expanding the mercury of the thermometer. Or if

the change be gradual, the body, when freezing, or paſſing from a fluid to a ſolid ſtate, will, at its freezing point, preſerve itſelf as long a time warmer, than the ſurrounding medium, as 10, 100, or 1000 degrees of heat thrown into it would have enabled it to do, without any change of form, and *vice verſa* when the body is melting, or paſſing from a ſolid to a fluid ſtate, it will preſerve itſelf colder as long as 10, 100, or 1000 degrees of heat taken from it would have enabled it to do."

"The number of degrees by which a body becomes hotter or colder, upon changing its form, will be as the capacity of the ſolid to that of the fluid, ſuppoſing the quantity of heat x in them, to be the ſame in all, or to produce the ſame expanſion when the change takes place, or their freezing point to happen at the ſame degree of ſenſible heat. Let the quantity of heat in them be expreſſed by 100. If the capacity of the ſolid be to that of the fluid, as in the laſt inſtance, as 1 : 2, then there muſt be twice as much heat in the body when fluid as there was in it when cold, or 100 degrees of heat muſt have become latent. If the capacities be as 1 : 3, then the whole heat, when the body is fluid, muſt be 300, and the latent heat 200. If the capacities be as 10 : 11, then the quantiy 100 muſt be increaſed in that proportion, and the whole heat, when fluid, will be 110, and the latent heat 100. And univerſally the whole heat in the ſolid being 100, the latent heat in the fluid will be equal to the difference of

the capacities of the ſolid and fluid, divided by the number expreſſing the capacity of the ſolid, and multiplied by the whole heat of the ſolid. Thus when the capacities are as 1 : 2, their difference is 1, which divided by 1 quotes $\frac{1}{1} = 1$, and this multiplied by 100 the whole heat of the ſolid gives a product of 100 for the latent heat. Here the latent heat is equal to the abſolute heat of the ſolid. When the capacities are as 10 : 11, then the difference 1 divided by 10, the capacity of the ſolid quotes $\frac{1}{10}$, which multiplied by 100, the whole heat of the ſolid at the melting point, gives 10 for the latent heat in this inſtance."

"Hence if the quantity of heat x be different in different bodies, this proportion will ſtill expreſs what the latent heat is. Thus in the laſt caſe, the latent heat will be $\frac{1}{10}$ of the whole heat in the ſolid, at whatever point the body melts. And hence alſo, if the melting point alter in water, the latent heat will alter likewiſe, and be greater if that point be higher, and leſs if lower; and ſo in ſpermaceti, wax, tin, &c."

This extract cannot be extended farther. With regard to the laſt point, ſome explanation may be neceſſary. It is ſtated that the latent heat ought to diminiſh with the lowering of the melting point of any body. A conſideration of the principles of the theory will readly ſhew, that every thing elſe remaining as before, the latent heat muſt diminiſh with ſuch a depreſſion of the melting point, otherwiſe the quantity of the abſolute heat in the body could

not bear the juſt proportion to its capacity. For example, the latent heat of ice being 140°, if that ſubſtance could be made to melt at a point below 32°, the latent heat would be expreſſed by a number leſs than 140°. If the melting point of ice could be made higher than 32°, the quantity of latent heat in the water produced would be greater than 140°. The ſame obſervations apply equally to the proceſs of vaporization, in which caſe, if the boiling point be altered, the latent heat of the ſteam ought to vary in the ſame way, But it is clear, that inſtead of this alteration of the latent heat, another in the capacities might take place, either ſufficient to counterbalance that, or even to alter the latent heat in a manner directly the reverſe. In effect, ſomething of this kind certainly takes place in the inſtance of ſteam. Water boils, it is well known, at a much lower point in vacuo, than under the preſſure of the air. Mr. Watt found the latent heat of ſteam in theſe circumſtances to be greater. To rectify the proportions, the capacity of this ſteam ought to be greater than uſual, or that of water leſs. There is reaſon to ſuppoſe that the ſteam found in vacuo has really a greater capacity than common ſteam. At leaſt, we generally, if not univerſally find, that expanſion not directly or ſimply produced by the operation of caloric, is accompanied by an increaſe of capacity. How the water is affected by diminution of preſſure is not ſo evident. But from ſome rude experiments which I made ſome time ago in the air-

pump, there ſeemed to me to be ground to ſuppoſe that the capacity of water even is a little increaſed in theſe circumſtances. My experiments were made with a view to determine whether any relation ſubſiſted between the expanſion of water in the air pump, as obſerved by Mr. Canton, and an alteration of the capacity of the water for heat. If the capacity is increaſed, the expanſion is rated too low by Mr. Canton. But if unexpectedly it had appeared that the capacity was diminiſhed, a totally new theory of the expanſion of water in the air-pump would ariſe, ſince the increaſe of temperature would be ſufficient to account for that of bulk. With this view, therefore, I repeated Mr Canton's experiments, with the addition of a thermometer placed in the water. But cold is uniformly produced, partly no doubt from evaporation, which proceeds rapidly in this caſe, yet probably in part alſo from an increaſe of capacity. That this is ſo, I inferred from the ſudden though ſmall riſe of the thermometer immerſed in the water, when the air was re-admitted. For it does not appear, that any inſtantaneous change of temperature could take place from the alteration of the temperature of the air alone.

What has been attempted to be explained in the laſt paragraph, may perhaps be made more diſtinct by help of numbers. If the capacities of ice and water be 9 and 10, the abſolute heat of ice 1260°, and the latent heat 140°, it is evident that when ice melts below 32° by any number of degrees, ten for

inſtance, that its abſolute heat becomes leſs, and is only 1250°. But the abſolute heats of bodies are as their capacities. Therefore, ſince 1250 is not to 1390 as 9 is to 10, either the abſolute heats or the capacities muſt be changed to render the proportions juſt. If the abſolute heat of ice melting at 22° be 1250, the abſolute heat of water at 22° would be one-ninth part greater, or 1388.88, and conſequently the latent heat would be equal to the difference between 1250 and 1388.88, or 138,88. But if 140° continued to be the latent heat, then the capacities muſt neceſſarily vary, and be as 1250 to 1390, or as 125 to 139.

The latent heat of any body, and the capacity of its ſolid and fluid forms being given, the quantity of caloric in the body in a ſolid form may be found. For ſuppoſe the capacities to be in the ſame proportion in all bodies, that of 1 : 2. then as the latent heat increaſes, the abſolute heat increaſes alſo; for in this caſe, the latent heat is always equal to the quantity of caloric in the ſolid body. But if the latent heat be the ſame in all bodies, the quantity of the abſolute heat in the ſolid varies with the capacities. Thus if the capacities be as 1 to 2, the latent heat equals the ſenſible heat as before; if as 1 to 3, the whole heat equals one half of the latent heat; if as 10 to 11, the whole heat equals ten times the latent heat. And univerſally, the whole heat will be equal to the capacity of the ſolid multiplied by the latent heat, and divided by the difference of the capacities.

cooled to this point by any ordinary means, till they had loſt their attraction for caloric.

Fluids require more caloric to raiſe their temperature to the ſame degree than their correſponding ſolids. If this were not the caſe, if the ſolid required more heat for this purpoſe than the correſponding fluid, we could not render that body ſolid by diminiſhing the temperature, but might render it ſo by raiſing it These bodies, if there be any ſuch, would in their own nature be fluid, in the greateſt degree of cold. If a body be capable of more changes in capacity than one, it will have more melting or freezing points than one, and we ſhall have the body in a ſolid form with different properties, and with ſimilar differences when it is fluid. Whether there are any ſuch is uncertain. The ſerum of the blood indeed freezes at 25°, where its capacity becomes ſmaller. But it recovers its capacity and fluidity when heated again above 25°. It becomes ſolid again if the temperature is raiſed above 156°, and as this is not performed inſtantly but gradually, its capacity ſhould again become greater, and latent heat go into it to make it ſolid. It does not, however, recover its capacity upon the heat being diminiſhed below 156°, but retains its ſolid form in all temperatures below this. Its appearance in this ſtate is very different from that of the ſolid produced from ſerum at 25°. If the heat be increaſed much above 156°, it again becomes fluid, but with different properties. Serum of blood, however, is

not a homogeneous ſubſtance, and theſe changes are probably connected with ſome chemical action among its component parts.

Sulphur alſo, in its liquid form, exhibits ſome phenomena, in its relations with caloric, which may poſſibly be connected with repeated changes of capacity. It has been long known to become very liquid at its fuſing point, but at a higher temperature to thicken remarkably. I propoſe, however, to allude to this circumſtance in the third part of theſe Eſſays.

This method of inveſtigating the natural zero has, as might be expected, attracted conſiderable attention from philoſophers. Objections have been ſtarted to various parts of the argument, and ſome of the later inquirers have been completely ſucceſsful, at leaſt in convincing themſelves of its fallacy. At the ſame time, it has not thus been uniformly attacked, and I might add a liſt of its adherents, which ſhould include many of the moſt candid and moſt diſtinguiſhed of the philoſophical world. In the ſucceeding part of this eſſay, it is my intention to conſider the nature, grounds, and importance of theſe objections, and to anſwer them, and remove or explain doubtful points, as far as I ſhall be able.

The objections offered to the doctrines in this and the foregoing eſſays have been of two claſſes: they either tend to ſhew its fallacy, from the reſult of experiments made on various ſubſtances, to determine the minimum of heat, and which have ge-

tainly given points of depreſſion for the natural zero, as wide of each other, as the warmeſt opponent of the doctrines could deſire; or they attack the principles upon which it depends with direct arguments of their falſehood, or ſuppoſed deductions leading *ad abſurdum*. As far as I have been able to conſider the ſubject, none of theſe arguments will reſiſt a ſtrict ſcrutiny.

I ſhall now proceed to recapitulate them in order, pointing out where they appear to me to be defective. The more the ſubject is inveſtigated, the more probability is there of attaining the grand end of all philoſophical inquiries—the diſcovery and promulgation of truth. If this method of finding the natural zero be really wrong, it would be better that it were completely decided to be ſo. But it would not be better to abandon it upon inconcluſive grounds, nor would the true intereſt of ſcience be promoted by ſuch a meaſure.

One of the principal cauſes of the doubt of ſome philoſophers with regard to Dr. Irvine's theory has ariſen from the very conſiderable, indeed extravagant diſcordance of the experiments and determinations of the natural zero. It ſeems at firſt ſight naturally and fairly enough ſtated, that this theory, which propoſes a method of diſcovering at what point a body ſhall be deprived of all heat, ſhould, upon reference to facts or experiments, be found to coincide with them. This, however, is totally impoſſible, for if it were to coincide with one ex-

periment, that would only lead it the further from the next. But in this inquiry, it is not to be forgotten that we have no authority for the accuracy of theſe experiments. We may relieve ourſelves from the dilemma equally well, by denying the truth of the experiments as that of the theory. Let us proceed to inquire which of theſe it is moſt juſt to do.

To form the ground-work of ſuch an examination I here ſubjoin a ſtate of the various points of depreſſion found by different authors for the natural zero. Among theſe I do not include that of Dr. Irvine, which was about — 900 in the double inſtance of ice and water, and ſulphuric acid and water. I omit theſe becauſe I know he was far from putting that implicit confidence in his experimental obſervations which ſo many recent authors have the happy faculty of obtaining.

TABLE OF THE NUMBERS

Expreſſing the natural Zero according to various authors.

Dr. Crawford	— 1500
Mr. Kirwan	— 1318
M. Lavoiſier	— 3426
Ditto	— 7260
Ditto	— 2598
Ditto	$-\frac{1852}{5171}$ below 32°

The laſt of theſe numbers is fractional, and expreſſed in degrees of Reaumur's thermometer. It

is beſides a negative quantity. I do not know exactly how it was obtained, nor what objections there may be againſt the proceſs by which it aroſe. But it is juſtly remarked by Séguin that it would prove the fallacy of the hypotheſis from which it is derived if the determination of the capacities were rigorouſly exact. This exactneſs, however, there ſeems very little reaſon to admit, as it ſhall be our object preſently to prove.

It is clear that the natural zero ought to be at the ſame point in all bodies; that is to ſay, it muſt require the like number of degrees to raiſe any body to a given temperature. But theſe degrees, though numerically equal, are very different with regard to the quantity of caloric which they mark. In order then that all bodies ſhould appear to contain an equal number of degrees of heat at the ſame thermometrical point, it is abſolutely eſſential that theſe degrees in each body be computed according to its own capacity. For example: if the natural zero be calculated in the caſe of ice, taking the capacities of ice and water as 9 and 10, and the latent heat as 140°, the number obtained will be 1260. But this number cannot poſſibly coincide with any other determination, becauſe theſe degrees are meaſured by the capacity of water. But they ought to have been proportional to that of ice, and the natural zero properly ſtated would thus be 1400°. It is by no means evident that this precaution has been duly attended to in conſtructing the tables of the real

zeros from which ſo many inferences have been drawn.

If the experiments above ſtated could be ſuppoſed to be perfectly accurate, and the arithmetical calculations juſt, the queſtion might be regarded as decided. But before admitting ſuch concluſions, it may be proper to pauſe a little and examine the ſteps by which theſe inferences were attained. In doing this, I ſhall firſt proceed to examine the various zeros found by Meſſrs. Lavoiſier and Laplace, as obtained by ſimilar and queſtionable means, and as differing the fartheſt from each other as well as from former determinations. The experiments of theſe gentlemen were performed by help of the calorimeter, an inſtrument for meaſuring heat invented by themſelves, and of which a full deſcription is to be found in Mr. Lavoiſier's Elements of Chemiſtry. The quantity of ice melted is ſuppoſed to furniſh preciſe grounds for computing the quantity of heat abſtracted from the ſubſtances under examination. Now the truth of the experiments in queſtion depends entirely upon the accuracy of this inſtrument. It is not a common accuracy that is required in this inſtance at leaſt, when the reſults are employed to inveſtigate the truth of Dr. Irvine's method of finding the natural zero. It will be ſeen that if the errors of the calorimeter amount to as much as a very ſmall fraction of the whole, the inferences are confeſſedly unjuſt. But even in a theoretical view, very ſerious objections may be made againſt this way of deter-

incided, granting the theory to be juſt; in the preſent ſtate of inſtruments for meaſuring caloric, no reaſonable man could poſſibly look for accuracy ſo great as certainly to exclude errors of one-fortieth part. Some of M. Lavoiſier's materials, as the nitrous acid, ſeem liable to many objections in all ſituations from the volatility of the acid. It may be worth while in a much eaſier caſe, that of ice and water, to compare the reſults in the hands of different philoſophers.

If in this inſtance, we call the latent heat of water with Profeſſor Wilcke* 57° 33 of Reaumur, or in Fahrenheit's ſcale, nearly 129°; and if with Dr. Irvine, we take the capacities of ice and water to be to each other as 8 to 10, the natural zero computed according to theſe grounds will be found to be 516. Theſe are the loweſt terms ſtated by authors. Let us now take the higheſt, and ſuppoſe with Mr. Kirwan, the capacities of ice and water to be to each other, as 9 to 10, and the latent heat to be as ſtated in Mr. Nicholſon's Syſtem of Chemiſtry 146°,† the natural zero turns out to be 1314° below 32°. Now, I appeal to every candid mind, whether the diſcordance of theſe reſults would not be regarded as complete proof of the falſehood of the theory, ſurely as much ſo, as the reſults already detailed, if the experiments had been made upon different ſubſtances. But they are upon

* *Annales de Chemie*, Vol. 3d, page 171.

† Page 15, Edition, 1796.

the ſame, and it became neceſſary for thoſe who diſcredit the theory, to refer the error to the experiments. This affords a ſufficient illuſtration of the great effect of apparently ſmall errors in theſe computations. But it is impoſſible to explain thoroughly the great difficulty of all experiments upon heat, a ſubſtance or property ſo fugacious and ſo eaſily tranſferable to all bodies that it cannot be confined or meaſured with unobjectionable accuracy.

It has been ſaid, that if we deny the truth of theſe experiments, what foundation do we leave for the theory which reſts upon ſimilar experiments? To this it may be anſwered, that we only deny the perfect accuracy of experiments, but by no means the approximation to truth within a certain degree. They may be ſufficiently near the truth to ſhew that certain properties of bodies exiſt, but yet ſo far removed from complete preciſion, as to make it impoſſible for the computed zeros to coincide; in a word, in the one caſe we need not be within a fortieth of the true number, in the other caſe, it is eſſential to be ſo. Surely there is a wide and fundamental difference between, on the one hand, obſerving that bodies have various capacities for heat, that they abſorb heat during fuſion, and at the ſame time have their capacities enlarged, and on the other hand, endeavouring to expreſs the latent heat and the ratio of the ſpecific heats in accurate numbers. We may come very near the truth; we may ſee clearly

that the properties in queſtion exiſt, and we may draw legitimate inferences from ſuch a data. But ſurely it is not conſiſtent with any maxims of ſound philoſophy to ſay, that we can know nothing at all, becauſe we are not in poſſeſſion of a perfectly accurate and preciſe knowledge. We perceive that any body melting abſorbs latent heat; we ſee that its capacity is augmented, and we conclude that the abſolute heat of the body is in a certain ratio to theſe properties. We further aſſert, that if theſe numbers could be accurately found, the whole heat would be found alſo. And whether we diſcover or do not diſcover theſe points, the deductions are equally true.

It requires but a ſlight inſpection and compariſon of the tables compiled by different authors, to convince any perſon, that we are far from being able to give accurate numbers for the ſpecific heats of bodies. Let us take Lavoiſier's acknowledgment, that errors equal to one-fortieth part of the numbers expreſſing the capacities would remove all objections, and let us proceed to conſider whether ſuch errors do not commonly occur. At leaſt it can be demonſtrated that authors differ from each other greatly more than to this amount, and all cannot be right. In the following table are uſed, partly the numbers expreſſing the capacities of thoſe ſubſtances, which have been employed for the determination of the natural zero, partly thoſe of the capacities of other bodies.

TABLE

Exhibiting a comparative View of the Differences of a few of the Numbers expressing the Capacities of various Bodies by different Authors.

Substance	Capacity	Author	Difference of capacities.
Ice	0.8 0.9	Irvine Kirwan	$\frac{1}{9}$
Sulphuric acid	0.4290 0.3345	Crawford Lavoisier	$\frac{1}{7}$ plus
Iron	0.1269 0.1430	Crawford Irvine	$\frac{1}{9}$ plus
Mercury	0.031 0.310	Crawford Thomson	$\frac{9}{10}$

These examples, which might be eaſily extended to a much greater length, to a length which ſhould include every ſubſtance of which the ſpecific heat has been examined by more than one perſon, will ſuffice to ſhew with what juſtice an appeal is made to the accuracy of experiments. In fact, there is no body of which the capacity can be aſſerted by the boldeſt believer to be determined nearly within a fortieth part of the true number. Much more concurrence, and experiments made with very large quantities of the materials, are indiſpenſably required before any juſt concluſions can be drawn from the reſults with regard to the natural zeros. It appears to me then, that theſe arguments may be regarded as ſufficient to prove, that the truth of this method of inveſtigating the abſolute heat in bodies is not

affected by the apparent diſcordance of ſome experimental inquiries.

We may now proceed to conſider the ſecond claſs of objections, thoſe which affect the principles of the theory. The late learned Dr. Robinſon, in his ingenious Notes on Dr. Black's Lectures, has ſtated on this ſubject, that the theory evidently preſuppoſes the conſtancy of ſpecific heats in low temperatures, or if they are not conſtant, that we know the laws of variation, and that theſe aſſumptions are highly improbable. I muſt differ from that learned gentleman with regard to the improbability. But as to the neceſſity of the conſtancy of ſpecific heats to the theory, I abſolutely deny its exiſtence, and have already endeavoured, in the eſſay on Capacities, to ſhew, that it is by no means neceſſary or eſſential to our knowledge of the abſolute heat in bodies. The analogical reaſoning from the phenomena of chemical attraction is, in this inſtance, far from being well founded. Dr. Robinſon, in the ſame place, adds, that it follows from this ſuppoſed inconſtancy of ſpecific heats, that even if it be granted that the heat given out during the freezing of water is the difference between the abſolute heat of the ice at 32° and that of the water at the ſame point, ſtill we cannot aſcertain the natural zero. There is here ſtated a doubt, whether the latent heat of water is the difference between the whole heat of the ice and that of the water at 32°. I am at a loſs to ſee how this can be denied. Ice at 32° contains, it will be al-

lowed, ſome portion of heat; whatever that is, we know alſo experimentally, as clearly as experiment can ſhew, that 140°, or thereabouts, enter the ice during its converſion into water. Perhaps I may be miſtaken in ſuppoſing any doubt on the ſubject, but if there is, I confeſs, I am unable to underſtand the grounds upon which it is founded. I cannot help remarking alſo, that the aſſertion that Dr. Irvine could not make his experiments on congelations for the diſcovery of the natural zero coincide tolerably, and that theſe differed enormouſly from the reſults of experiments on mixtures is not perfectly juſt, as far at leaſt as it is meant to apply to the experiments performed by Dr. Irvine himſelf. The truth is, that Dr. Irvine, in both theſe methods, made the natural zero nearly —900. But he was far from putting that implicit confidence in the accuracy of his obſervations that many philoſophers have ſince done, and did not conſider his experiments as by any means concluſive upon theſe points.

M. Seguin, in the *Annales de Chimie*, vols. 3d and 5th, has ſome obſervations of much ability upon this ſubject. I have already taken notice of the account that he there gives of the experiments of Meſſrs. Lavoiſier and Laplace. After remarking that theſe experiments are not deciſive, he proceeds to endeavour to demonſtrate the inſufficiency of the theory of capacities to determine the point of privation of all heat. In doing this, he lays down three propoſitions:

I. Caloric in certain circumſtances obeys the laws of affinity, and combines with the molecules of bodies.

II. Capacities are not permanent.

III. The abſolute caloric is not proportional to the ſpecific caloric.

At leaſt M. Seguin contends that all theſe three propoſitions do not exiſt at once. But they are all neceſſary to the doctrine of capacities: hence he infers, that doctrine cannot be true.

M. Seguin has certainly treated his ſubject with much ingenuity, and has collated into one view the various objections to the mode of accounting for the production of heat and cold, attributed by him and many of the continental philoſophers to Dr. Crawford, but of which Dr. Irvine was the real author. The firſt of his propoſitions regards the entrance of caloric into the molecules of bodies by the laws of affinity. This queſtion is, however, far from being ſo eaſily ſettled as M. Seguin ſuppoſes. Whether caloric does or does not obey theſe laws, muſt be conſidered as very doubtful. It is extremely uncertain if it really be matter at all. If it is not, it cannot be ſuppoſed to be under the influence of attraction. Even if caloric be material, it does not follow that it ſhould. It is at beſt but a plauſible conjecture, and depends altogether on another conjecture in itſelf in need of proof. It is impoſſible to admit with Seguin, that caloric united with his molecules does not affect the temperature of bodies,

and that conſequently the real zero would not mark a total privation of heat. The quantity of caloric contained in a body, is known by its ſpecific heat as well as by its temperature. If the change of capacity be the term at which heat is ſuppoſed to enter into the molecules, as in the caſe when ice at 32° is converted into water at 32°, we affirm that the temperature compared with the capacity, ſhews the abſolute quantity of heat in the water as much as in any other inſtance. For the temperature of no body affords grounds to judge of its abſolute caloric, unleſs account at the ſame time be alſo taken of its ſpecific heat. When the capacity of a body does not change, either no heat enters into the molecules, or it goes in and comes out again according to a rule or ratio which we call the ſpecific heat of that ſubſtance. Caloric never enters into any body without either raiſing its temperature or increaſing its ſpecific heat. To this general fact, too much attention cannot be paid. There is no proof, and no inſtance can be given, where caloric combines with the particles of matter without doing one or other of theſe. Therefore, though caloric ſhould really combine with the molecules during fuſion, or at any other time, provided only, that it combines in a manner of which it gives notice by the raiſed temperature or increaſed ſpecific heat of the body, the determination of the real zero will not be affected by ſuch a combination. And, if by patient and ſkilful efforts, all the experimental determinations of this point

ſhould be brought to agree, we muſt not conclude, that caloric does not unite with the particles of bodies, but only that it does not unite out of proportion to their ſpecific heats. It is perhaps, impoſſible to demonſtrate the firſt of Seguin's propoſitions to be ill-founded, but the reverſe of it can be rendered highly probable, and it is in itſelf without poſitive proof. It is difficult to ſee any abſolute inconſiſtency between the doctrine of capacities and that of the chemical combination of heat. It is perhaps of little conſequence, whether we know the law by which bodies ſeize, or that by which they only receive caloric, provided we only know the ratio. This part of our ſubject is extremely hypothetical and unſatisfactory, and may be abandoned with advantage. We cannot expect any preciſe anſwer to almoſt any queſtion concerning the nature or mutual action of the ultimate diviſions of matter. The doctrine of particles is yet involved in a dark, and it is to be feared, an impenetrable cloud. Whether the rays of ſcience are one day deſtined to pierce this obſcurity it would be fruitleſs to gueſs and preſumptuous to determine. Yet, if the mutual action of three particles upon each other has puzzled and foiled the profoundeſt philoſophers of Europe, how ſhall we hope to determine that queſtion, which embraces the conſideration of theſe minute bodies, infinite in their number, endleſs in the variety of their magnitudes and their forms, and agitated by a thouſand motions, of which we know neither the cauſe nor the degree?

If then a body abſorbs heat in a regular way which we can inveſtigate and foretell, as has been done in many caſes, it is leſs neceſſary to inquire how this is performed. It is without all doubt neceſſary that caloric ſhould enter into bodies regularly, or in a way which we can predict. But if it enters in ſuch a manner, it is perhaps of inferior importance whether it does or does not combine chemically with the body. There are the ſtrongeſt grounds for imagining that whenever a body changes its capacity it takes in or gives out caloric in ſuch a way that the abſolute heat ſtill remains proportional to the capacity. At leaſt among the almoſt infinite variety of chemical phenomena, where heat or cold is produced, in every caſe the ſpecific heat ſeems to be accommodated to the alteration of the temperature, to become great when the temperature is diminiſhed, and leſs when it is increaſed. If on caſe could be ſhewn where this does not happen, the argument muſt be abandoned. But till that is fairly done, it may be allowed to ſay, that there is here a moſt marvellous coincidence, if it really has no connexion with the phenomena.

There may be conceived to be three caſes of the union of caloric with ſubſtances expoſed to its action. Caloric may enter either by chemical combination, or by ſomething different from this, which M. Seguin has called the admiſſion of heat between the particles of matter, or it may enter partly in both

ways. But our argument will remain unaffected whichever of theſe modes of union is really conſiſtent with facts, provided the heat can be diſcovered to enter according to any conſtant rule.

M. Seguin's ſecond propoſition, that the capacities of bodies are not permanent, I have already diſcuſſed, and ſhall not again enter upon here. I have endeavoured to ſhew, that the capacities probably are permanent, and that we have no juſt grounds to doubt this fact; but if they even are not ſo, that the theory is no wiſe dependant on ſuch a ſuppoſition.

In like manner, the third propoſition, that the abſolute calorics are not proportional to the ſpecific, I have already fully diſcuſſed.

M. Seguin, after acknowledging that the experiments of Meſſrs. Lavoiſier and Laplace, the reſults of which I have already ſtated, are not ſufficient to determine finally the queſtion of finding the natural zero, proceeds to draw deductions of a ſimilar kind, but, as he thinks, infinitely more ſtrong, from ſome experiments upon the combuſtion of phoſphorus, hydrogen, and carbon. The details of theſe are to be found in Lavoiſier's Elements of Chemiſtry. It was found that each pound of oxygen during the combuſtion of phoſphorus in it, diſengaged enough of caloric to melt 66.66 pounds of ice. Whence, ſuppoſing the oxygen to have parted with all its heat, and the water formed to contain 135° of latent heat, there muſt have been in the oxygen 8999° of calorie

in degrees meaſured by the capacity of the water. Or taking Dr. Crawford's determination of the capacity of oxygen at 4.769, that number applied to oxygen is reduced to 1887 nearly, which is the real zero according to the calculation. There are ſeveral circumſtances to be taken into account before admitting the truth of this inference. It is to be recollected, that the experiment was made by burning the phoſphorus in the calorimeter. All the objections to the accuracy of that inſtrument bear with full force upon this queſtion. But even if that point were conceded, one yet more material remains. The whole of this reaſoning hangs upon the juſtneſs of Dr. Crawford's determination of the ſpecific heat of oxygen. If this be not fairly found, no argument can be drawn againſt the general theory or the application of it to diſcover the real zero. A very moderate alteration of the number expreſſing the capacity of oxygen would render this zero nearly the ſame with that of Dr. Crawford's own determination, from which indeed even in its preſent magnitude it does not very widely diverge.

Another determination of the natural zero is made by M. Seguin from the reſult of the combuſtion oxygen and hydrogen. It depends on ſimilar principles to thoſe already laid down. The firſt aſſumption is the accurate determination of the quantity of heat in vital air, to which may be added that of the preciſion of the calorimeter as a meaſure of heat. Such an accident as the temporary freez-

ing, or any impediment in the courſe of the water in the conducting tube would readily account for a much greater quantity of caloric than what is here conſidered as exiſting in the diſcharged water. After all, the zero thus determined amounts only to 1612.72.

The experiments upon the combuſtion of carbon in oxygen are yet more hypothetical. For in that caſe, beſides the former conſiderations, the capacity of carbonic acid is ſuppoſed to be aſcertained. Upon the whole, it may be obſerved, that theſe objections apply rather to Dr. Crawford's ſtatement of the capacities of different gaſes, and other bodies applied to the explanation of the ſource of animal heat, than to the general doctrines delivered in theſe Eſſays.

Of all ſubſtances none ſeem leſs fitted to afford an accurate knowledge of their relations with heat than the aëriform bodies. The difficulty of conducting experiments upon them is very great, while the ſources of inaccuracy are unuſually numerous. The purity of theſe ſubſtances is far from being eaſily aſcertained, and they may readily give origin to erroneous ideas concerning heat from their contamination with various other airs, with ſolid ſubſtances which they can retain in ſolution, and from the very moiſture which in an unavoidable but uncertain quantity never fails to accompany all gaſeous bodies. One cannot help remarking here the extreme and extraordinary accuracy of M. Lavoiſier's reſults. He never ſeems to ſuffer any loſs in his experiments,

or at leaſt only ſome evaneſcent fraction of a grain. That philoſopher's high merits in every reſpect would alone protect him from any reflection of inaccuracy. Yet the mind is involuntarily ſtruck with a combuſtion of hydrogen and oxygen, where ſix pounds ten ounces five gros and twenty-four grains of the gaſes are uſed, and ſix pounds ten ounces five gros and twenty-four grains of water produced. If this experiment would bear repetition with the ſame accuracy, it would afford the beſt poſſible argument againſt the gravity of caloric.

The only other objections to this method of diſcovering the loweſt degree of heat which occur to me as worthy of conſideration are thoſe urged by Dr. Thomſon, in his Syſtem of Chemiſtry. I have already noticed his obſervations upon the theory of capacities in general, and cloſely as every part of that theory is connected, it cannot be neceſſary to enter again upon this ſubject at great length. That gentleman, in the laſt edition of his work, vol. I. page 398, has inſerted the following paſſage.

"If the ſpecific caloric of bodies has any meaning, it can only be, that the ſame quantity of caloric raiſes the temperature of one body a greater number of degrees than it does another. When we ſay that the ſpecific caloric of A is = 6, and that of B = 1, what do we mean, unleſs that the quantity of caloric which raiſes B 6° raiſes A only 1°, or that what raiſes B 60° or 600°, raiſes A only 10° or 100°? When we we ſay that the ſpecific caloric of water is 10°, and

that of ice 9, do we not mean, that the quantity of caloric which raiſes the ice 10°, or 100°, raiſes water only 9°, or 90°? Yet during the change of ice into water 140° of caloric enter it without raiſing its temperature; *a quantity greater than what can be accounted for by the difference of ſpecific caloric by 126 degrees.* When water is converted into ſteam, 1000° of caloric diſappear; yet the ſpecific caloric of ſteam is to that of water, according to Dr. Crawford's own experiments, only as 155 to 100; *ſo that no leſs than 483 degrees of caloric diſappear, which cannot be accounted for according to this theory.*"

If this reaſoning were well founded, the theory would indeed require no further confutation. But it is not very difficult to ſhew the ſource of error in this argument. The 126 degrees here mentioned, were obtained by Dr. Thomſon, as we learn from a former work of his, in the following manner. He ſays, let the 140 degrees of latent heat in water be ſuppoſed to be only degrees meaſured by the capacity of ice, inſtead of that of water, to which they are really proportional. When therefore theſe degrees enter ice at 32°, the temperature ought to be 158°: for 10 : 9 :: 140 : 126, but it was only 32°. Therefore 126° of caloric have, as Dr. Thomſon aſſerts, diſappeared, and cannot be accounted for by the change of ſpecific caloric.

Dr. Thomſon thus gives it as a fair ſtatement of Dr. Irvine's theory, that the 140° entering ice during its change into water, ſhould be leſſened in the

ratio of 9 to 10, and ought therefore to be only 126°; or in other words, if the heat entering the ice were only sufficient to raise the temperature of the ice 140°, the temperature of the water ought to be 158°. On this supposition, however, the water would contain two portions of heat, its original quantity from the natural zero to 32°, expressed in degrees measured by the capacity of ice, and the superadded portion from 32° to 158° measured by that of water. The ice, according to this statement, would no doubt melt without absorbing any caloric, and the water would be merely heated as in every other case. But Dr. Irvine, and after him Dr. Crawford, and the writers of Elementary Treatises, and I believe I may say all our philosophers, except Dr. Thomson, have stated this doctrine of capacities to be, that every one of the degrees, expressing the whole heat contained in a body is to be taken in proportion to the capacity of that body. And therefore that if ice suddenly changed its capacity it would absorb not merely a rateable proportion of what heat might be presented to it, but an absolute quantity to make up for its new capacity. For the quantity of heat necessary to raise the temperature of ice each degree from the natural zero, is to the quantity of heat necessary to raise the temperature of water each degree from the same point, as the capacity of the one body is to that of the other. Ice cannot then, as it acquires its new form, shew any increase of temperature, till

the differences between the heats of each degree from the loweſt point be made. Theſe differences amount to the 140° found by experiment, and cannot raiſe the temperature of the water even by the fraction of a degree, becauſe it is barely ſufficient for the demands of its new capacity. Had the 140° been applied to water, Dr. Thomſon's aſſertion of the uſe of temperatures would have been juſt, but it can by no means be admitted, when alluding to a portion of caloric entering a body during a change of capacity.

It is obvious, that if we could inſtantaneouſly increaſe the capacity of any body, it would immediately become colder; and its temperature would ſink proportionally as much lower as its new capacity was higher. In the ſame manner if a body has its capacity ſuddenly increaſed, and at the ſame time a quantity of heat added to it to make the abſolute caloric proportional to its new capacity, that body would continue exactly at the ſame thermometrical point. But melting ice in ſuch a body, the 140° are demanded by its new capacity, they would have been more, if its capacity had been greater, and leſs if leſs. On the very principles of the theory, therefore, ice, on becoming water, ought to receive a quantity of heat, and that quantity is not governed by the proportion of 9 : 10, but the whole heats are in that proportion, and the 140 degrees are only their difference. All calculations on the alteration of tem-

perature to be produced by a given portion of caloric, a body from the knowledge of the relation of its capacity to that of another body, and of the number of degrees that other is raiſed by the ſame quantity, continue juſt only while the capacities continue in the ſame proportion. Dr. Thomſon's obſervations upon ſteam are equally objectionable, and are to be anſwered preciſely on the ſame principles. It would be needleſs waſte of time therefore to extend further the conſideration of this ſubject.

Immediately connected with the theory of heat which it has been my object to explain and illuſtrate in theſe Eſſays, is the account given of the origin of animal heat and of combuſtion, by Dr. Crawford. It is indeed ſo obviouſly dependant on, and ſo immediately derived from the doctrine of capacities, and particularly from that part explained in the following eſſay, that I am a little ſurpriſed to obſerve it aſſerted by ſeveral authors, ſome of whom are very recent, that Dr. Crawford's theory is derived from Dr. Black's ſpeculations, when ſurely it evidently comes directly from the doctrines now treated, and only through them from the theory of latent heat. It wholly hangs on the previous admiſſions, that bodies contain heat according to their capacities, and that a change of capacity is always accompanied by a proportional change of temperature, propoſitions firſt, as I believe, enunciated by Dr. Irvine, certainly

neceſſary to his theory, and neither of which were, nor could be conſiſtently, propoſed by Dr. Black. The merit of the laſt named philoſopher does not require, and ſcarcely admits of increaſe. Nor is it my wiſh that any pretenſions of Dr. Irvine ſhould encroach on the well-eſtabliſhed diſcoveries of Dr. Crawford. If any unfortunate differences exiſted between theſe two philoſophers, they are beſt committed to oblivion. But that Dr. Crawford himſelf was of opinion that his doctrines were derived from Dr. Irvine's may be inferred from the following paſſage in the *firſt* edition of his work on Animal Heat, page 17, where, after having detailed the general facts and inferences regarding caloric, Dr. Crawford proceeds to lay before his reader his experiments and obſervations on animal heat, and the inflammation of combuſtible bodies.

"I think it proper to obſerve that I was led to the conſideration of this ſubject in conſequence of having attended the chemical lectures of the learned and ingenious Dr. Irvine of Glaſgow. And it is a tribute of juſtice which I owe to this philoſopher, to acknowledge, that the ſolution which he has given of Dr. Black's celebrated diſcovery of latent heat, or of the heat which is produced by the congelation of water, ſpermaceti, bees' wax, metals, &c. ſuggeſted the views which gave riſe to my experiments.

"I muſt alſo add, that Dr. Irvine, from the ſo-

lution which he has given of the above phenomena, and from the general fact, that heterogeneous bodies have different capacities for containing heat; concluded, that there was a difference between the absolute heat of fixed and atmospherical air. It remained to ascertain this difference, and to determine by experiment, whether fixed or atmospherical air contained the greater quantity of absolute heat."

And in the second edition of Dr. Crawford's work, at page 10, in a note, it is observed, that the general fact respecting the influence of a change of capacity, upon the temperature of bodies, was first discovered by Dr. Irvine. These quotations are sufficient to shew, that the theory of animal heat and inflammation proposed by Dr. Crawford, was in the opinion of that gentleman himself founded upon the previous discoveries of Dr. Irvine.

I now conclude my endeavours to state and explain the opinions of my father upon the subject of caloric. Whether I have in every instance succeeded in giving his sentiments correctly, the distance of the time, the imperfection of the manuscripts, the inaccuracy of recollection, prevent me from ascertaining. It has, however been my object to produce as clear and consistent an account of my subject as I found it possible to do, and I leave it in the hands of the reader with a hope, I trust not ill founded, that he will be more ready to display his candour than his severity of judgment. Throughout these Essays it has been my endeavour to shew, that the

[redacted]

APPENDIX TO ESSAY IV.

Among the manuſcripts of my father there were found the following definitions and propoſitions. They are obviouſly incomplete, and were probably intended to have been enlarged, ſo as to contain the whole of his doctrines. Notwithſtanding this imperfection I have determined to inſert the portion finiſhed, ſince it appears to me to comprehend a condenſed and accurate account of a conſiderable part of the ſubject of caloric as viewed by Dr. Irvine.

That philoſophers may reaſon with diſtinctneſs and perſpicuity about heat, it is neceſſary that they ſhould agree in the terms which they uſe to expreſs the various meaſures which they apply to it.

The meaſures of heat known to us are, I think, reducible to three, to wit, temperature, relative quantity, and abſolute quantity.

I. By temperature of heat in a body is meant that

which is indicated by the thermometer, this is always measured by the degree which the thermometer is raised by that temperature.

Temperature therefore is always expressed by degrees of the thermometer, and we may say the temperature of one body is equal, is higher or lower than that of another; and if higher or lower we may express the difference by degrees of a certain thermometer, but, until the ratio between one temperature and another be ascertained by experiment and induction, we ought to consider temperature as a measure which admits of degrees, but not of ratios. Nor ought we to use the expression of a double or triple temperature, these being terms which can convey no distinct meaning until the ratio of different temperatures be determined.

II. As it is now known that two bodies of different kinds, even when equal in temperature and magnitude, or equal in temperature and weight, may yet have different quantities of heat, it is necessary to have some term by which this difference of bodies with relation to heat may be expressed and measured; and this is what I call relative heat, or relative quantity of heat. The relative heat of a body therefore is always measured by the ratio which the whole heat contained in a body bears to the whole heat of some body of
temperature and
ture

Thus if one meaſure of water contains as much heat as two meaſures of quickſilver of the ſame temperature, this is expreſſed by ſaying that the relative heat of water is double to that of the ſame bulk of mercury, or if one meaſure of mercury is equal in weight to fourteen of water, we may ſay that the relative heat of water is to that of the ſame weight of mercury as 28 to 1. Again, if ſeven pounds of water have as much heat contained in it as eight pounds of ice of the ſame temperature, this is expreſſed by ſaying that the relative heat of water is to that of ice as 8 to 7.

III. The abſolute heat of a body is the whole quantity of heat contained in it. This is doubled when the body is doubled, tripled when the body is tripled, while the temperature and relative heat are unchanged.

Corollary. From this and the ſecond definition, it follows, that the abſolute heat of a body A to that of another B of the ſame temperature, is in the compound ratio of their magnitudes, and their relative heat in relation to magnitude; or in the compound ratio of their weights and their relative heat in regard to weight; and their abſolute heats will be equal, when the magnitudes or weights are reciprocally as their relative heats in relation to magnitude or weight reſpectively. When I ſpeak of the quantity of heat without any qualification, I always mean the abſolute quantity.

PROPOSITION I.

The quantity of heat which raiſes a body one degree in temperature is half of that which raiſes the ſame body two degrees, one third of that which raiſes it three degrees, and ſo in proportion either accurately or very nearly. This proportion is proved by the experiment of mixing equal quantities of water or any other fluid of different temperatures; the mixture is found to have always that temperature, which is an arithmetical mean between the temperature of the bodies before mixture.

The reaſon why this proportion is expreſſed as either accurately or very nearly, is that it may be doubted whether the liquor in the thermometer has not by equal quantities of heat expanſions which bear the ſame ratio to the whole liquor, or whether it has preciſely equal expanſions.

Corollary. Different bodies are raiſed from one degree of temperature to another by a quantity of heat which is as the degrees of temperature added, the bulk of the body, and its relative heat taken conjunctly. For when the relative heat and bulk are the ſame, the quantity of heat given will be as the number of degrees of temperature added. When the temperature and relative heat are the ſame, the quantity of heat added will be as the

magnitude of the body; and when the temperature and magnitude are the ſame, the quantity of heat is as the relative heat. Therefore, when none of the three is the ſame, any quantity of heat added will be the compound ratio of the three; to wit, the degrees of temperature by which the body is raiſed, the magnitude of the body, and its relative heat in relation to magnitude.

PROPOSITION II.

Having given the magnitudes, the relative heat in regard to magnitude, and the temperature of any number of bodies which mix without any change of their relative heats, to find the temperature of the whole mixture:

Solution. Let a, b, c, d, be the given bodies, and let the ſame ſymbols expreſs their magnitudes: let d have the loweſt temperature, and let the degree of temperature of a, b, c, above d, be l, m, n, reſpectively: Let the relative heat of d be expreſſed by unity, and the relative heat of a, b, c, compared with that of d in equal magnitudes be x, y, z, reſpectively.

Multiply the ſeveral magnitudes a, b, c, firſt into their reſpective relative heats x, y, z, and then ſeverally into the degrees of temperature by which each exceeds the temperature of d, to wit, into l,

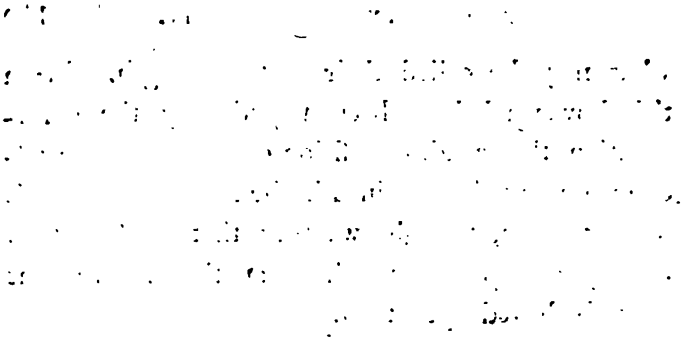
m, n, reſpectively. Add the three products together, and divide the ſum by the ſum of all the four magnitudes, and the quotient will give the degrees of temperature by which the mixture exceeds the temperature of d, which in the preſent example will be

$$\frac{alx + bmy + cnz}{a + b + c + d}$$

This ſolution follows neceſſarily from the corollary of the firſt propoſition.

To complete this mathematical ſtatement of Dr. Irvine's view of the theory of heat, ſeveral other propoſitions are obviouſly wanted. Of theſe, however, I have not been able to find any notes of the author's writing. That he did extend his ſpeculations in this form, and that he was partial to this mode of conſidering the ſubject, I have good reaſon to believe. He was accuſtomed to make his chemical lectures the vehicle of his opinions; and truſting to the clearneſs of his ideas and the accuracy of his recollection, he committed to paper comparatively few of his doctrines. I hope, however, that this portion which I have been able to ſelect will not be wholly unacceptable, though incomplete. It was at one time my intention to have endeavoured to extend theſe propoſitions to

what may be called their original length. From this however I have been diſſuaded by the conſideration that a full expoſition of almoſt all the additional points is contained in the foregoing and following pages, though wanting that mathematical brevity and preciſion, which another form might have beſtowed upon them.



PART THE SECOND.

ESSAYS

ON VARIOUS SUBJECTS.

BY THE LATE

WILLIAM IRVINE, M. D. F. R. S. Ed.

LECTURER IN MATERIA MEDICA AND CHEMISTRY IN THE UNIVERSITY OF GLASGOW.

" Labor indefessus, oculi intentissimi, et manus nigroremt carbonum non aversantes, precipue hi, magistri mihi fuerunt."

ESSAY I.

ON HEAT PRODUCED BY MIXTURE.

THE heat produced on many occaſions by mixing bodies, is obviouſly of the ſame nature with that obtained from the inflammation of combuſtible ſubſtances, and from animals. No effect whatever is expected from it, which the ſame degree of heat would not produce from whatever ſource it might be obtained. The heat thus excited is of very ſhort duration. It is greateſt at the moment the bodies are perfectly united, and from this gradually diminiſhes till the whole is cooled down to the temperature of the air. It is ſcarcely poſſible then to employ the heat produced in this manner in chemical operations, becauſe we cannot preſerve it for a ſufficient time of the ſame intenſity. Hence the heat extricated by mixture has been of little ſervice in diſcovering the effects which heat is diſpoſed to produce upon bodies.

Upon ſome occaſions, ſubſtances, upon being mixed, become colder than they were before. By this means more heat can be taken out of bodies than by any other hitherto diſcovered. By diminiſhing heat in this manner, its extenſive influence has been inveſtigated. It is by ſuch means that it has been experimentally proved, that quickſilver is really poſſeſſed of all the properties that are peculiar to metallic ſubſtances, and that it differs from all thoſe bodies hitherto known, in requiring only a very ſmall degree of heat to melt it. By mixing bodies in the greateſt degrees of cold that appear in nature a greater is produced, and there is reaſon to expect, if the experiments by which mercury has been frozen were to be repeated in the intenſe colds of Siberia, that more remarkable effects would be produced, and that ether, ſpirit of wine, and air, the only bodies that have never been ſeen in a ſolid form, might be there deprived of their fluidity, and the opinion that the fluidity of bodies depends on heat, receive the moſt complete experimental proof.

The degree, to which this diminution of heat can be carried, is limited, if the concluſions drawn from my experiments for determining the natural zero be well founded. Probably long before they are cooled to this point, all bodies would become ſolid. It would be ſingular, if the denſity in all were the ſame, when cooled to this degree.

Though the heat produced by mixing bodies has not been ſo uſeful in enlarging our ideas concerning

heat, as the diminution of it by mixture has been, it has, however, been much attended to by philoſophers on account of ſeveral curious circumſtances in its production. Heat or cold is produced by the ſame ſubſtances according to the ſtate they are in before they are mixed. In general it is by mixing two ſolids, or a ſolid and a fluid, that cold is produced. It is generally by mixing two fluids that heat is produced. Thoſe ſolids, or the ſolid and fluid which when mixed become colder, if they are rendered fluid before mixture, will produce heat upon being mixed. Thus nitrous acid mixed with ſnow or ice becomes inſtantly fluid, and a conſiderable degree of cold is produced. If the ſnow or ice is melted before it is mixed with the aqua fortis, a conſiderable degree of heat is the conſequence of the mixture.

The cauſe of theſe differences has excited the attention of the greateſt philoſophers. The ſingularity of the phenomena rather than any diſtant proſpects of utility, attracted their notice. They have differed very much from one another in the cauſe which they have aſſigned for theſe appearances. They do not differ only among themſelves; the explication given by any one of them is ſeldom conſiſtent throughout. From ſome experiments made ſeveral years ago, a way preſented itſelf of accounting for the heat produced in mixtures, different from any that had been thought of before, and at the ſame time, more agreeable to the phenomena. Many

others had before occured to me, but on a nearer view were rejected, becauſe they did not agree with the phenomena, and were not ſufficiently ſupported by experiment. So many had ſucceſſively preſented themſelves, and had then been rejected, that after a long and fruitleſs ſearch, I at laſt deſpaired of ſucceſs. I was about to leave inquiring after a ſubject which had hitherto been ſo barren, when accident ſuggeſted to me an explication, which, ſo far as I have examined hitherto, has the cleareſt experimental proof.

After all it may be ſaid that the ſubject was not worth the trouble, and that any diſcovery, if, after all, any diſcovery be made, which the ſubject could afford, was too dearly purchaſed; that it is at moſt but ſpeculatively curious, and has little chance of being ever of any uſe to mankind.

Theſe obſervations, which every one will naturally make, are no doubt true. But no perſon can tell to what purpoſe any experiments or diſcoveries may be applied. Many things which at firſt ſight had as much the appearance of idle curioſity, have furniſhed ſome of the moſt uſeful inventions. The perſon who amuſed himſelf with the glaſſes of a pair of ſpectacles, and obſerved that, in a certain poſition, they apparently turned objects upſide down, might be held as very idly employed, though the diſcovery of that uſeful inſtrument the teleſcope was the conſequence of it. Nor would one have imagined, that he who employed himſelf in rubbing a piece of am-

ber, and who was delighted with ſeeing the ſtraws dance around it, an occupation more like madneſs than philoſophy, was making an experiment, that one day would lead to the explanation of ſome of the moſt remarkable phenomena in nature.

In giving an account of this ſubject, I ſhall differ from philoſophers of the greateſt name, and for whoſe characters I have the higheſt regard. This will not, I truſt, be imputed to fooliſh vanity, but to that earneſt wiſh for the diſcovery of truth, which ought to take place of every other conſideration.

Fluid bodies, upon being mixed, are almoſt the only ones that become hotter. In a very few caſes a ſolid and a fluid produce a conſiderable degree of heat. The degree of heat produced by mixture is different in different bodies. In ſome it is ſo violent as to make the bodies burſt out into actual flame, though formerly as cold as the air in the coldeſt weather. On other occaſions bodies, when mixed, apparently do not become hotter. Even in theſe, however, heat is produced in a certain degree; but in order to perceive it we muſt uſe an inſtrument for that purpoſe. In thoſe caſes where the heat produced by mixture is ſo intenſe as to ſet the whole on fire, the bodies have ſo completely united, and ſuch other changes have taken place, that they cannot be again ſeparated.

Some bodies which, on being mixed produce a very conſiderable degree of heat, retain, however, after their mixture ſo many of their original pro-

perties, and are ſo ſuperficially united, that they can be ſeparated again ſo as to recover all their former qualities; ſo that on mixing them a ſecond time, they will produce the ſame degree of heat as at firſt. The moſt remarkable of this kind are the mineral acids when mixed with water. In all of theſe mixtures a conſiderable degree of heat is produced. None of the mineral acids, on being mixed with water, produce ſo great an increaſe of temperature as the vitriolic acid. When both ſubſtances before mixture are as cold as ice at the freezing point, on being mixed, the heat is greater than that of boiling water. The degree of heat in this mixture varies according to circumſtances. It is greater, when the acid is ſtrong than when weak. It is greateſt with the ſame acid, when the quantity of acid nearly equals that of the water.

The qualities of the compound of acid and water are a mean between thoſe of the acid and water before mixture. None of the principles of which either body is compounded has been loſt during the mixture, or has been changed into heat or light. The water and the acid can be again ſeparated from each other, and in this ſeparate ſtate will, upon being mixed a ſecond time, produce the ſame heat as at firſt.

The heat produced in this caſe cannot be accounted for, from any new arrangement of the principles which compoſe thoſe bodies, but muſt depend on ſome other cauſe.

It is the heat produced in mixtures of this kind which I principally intend to treat of at preſent.

The heat produced in thoſe caſes where the bodies are decompoſed, is more difficult to account for, and is more nearly allied to the manner in which heat is produced by the inflammation of combuſtible bodies. The production of heat is, however, probably, even in this caſe, partly owing to the ſame cauſe, which produces heat in the mixture of acid and water; that part of it I mean which is firſt produced, and which by diſpoſing the ingredients of the bodies to ſeparate, paves the way for the production of it in another manner. This, however, is but hypotheſis, and as ſuch, I wiſh to diſtinguiſh it. It is ſeemingly not eaſily capable of experimental proof, as we cannot get the bodies conſiſting of the ſame ingredients; after the heat has been produced as at firſt, and by comparing them together, diſcover whether in this reſpect they agree with the mixture of acid and water.

The heat produced upon mixing acid and water has been explained by authors in two ways, the one accounting for it from the violence with which the particles of bodies ruſh together, and the great friction which muſt thus neceſſarily ariſe. The other explanation of it is, that the bodies moſt diſpoſed to produce heat in mixture, are ſuch as are of a fiery and acrid nature, qualities which have been accounted for by ſuppoſing that a certain quantity of fire was contained in thoſe bodies, which meeting

the like. It is only in very compound fluids that motion produces heat, ſuch as milk, and the ſolution of certain ſalts in water. Milk, when agitated in the common proceſs of churning, becomes hotter, but in proportion as it heats, a quantity of butter ſeparates from it, and inſtead of being completely diſſolved and fluid, aſſumes a ſolid form, and the whole after the agitation is very different from what it was before. When a quantity of Glauber's ſalt is diſſolved in warm water, and the whole is allowed to cool without being diſturbed, upon ſhaking the ſolution it grows hotter, but a part that was fluid is inſtantly made ſolid. So little are the ſimpler fluids diſpoſed to produce heat by being ſet in violent motion, that they hinder ſolid bodies when their particles are moved from producing heat. A coach wheel which by being moved with great velocity round its axis without oil, will produce heat ſufficient to ſet the whole on fire, upon being oiled will produce little or no heat. If the effect of the oil be in this caſe to hinder any violent motion being excited in the particles of the ſolid bodies, it is equally evident, that from the great eaſe and readineſs with which the particles of fluids ſlide naturally over each other, that they are ſcarce capable of being thrown into any tremor, as the particles of ſolid bodies from their nature may eaſily be. It is not very certain whether, when the particles of ſolid bodies are ſet in violent vibratory motion, a quantity of heat can with much propriety be ſaid to be produced; as this ſeems to

imply its being brought at that time into exiſtence. Perhaps it would be more proper to ſay, when the particles of ſolid bodies are put in a violent vibratory motion, that a quantity of heat is collected from the ſurrounding bodies; in the ſame manner as a glaſs globe on being rubbed, and having a ſimilar vibratory motion excited, collects from ſurrounding bodies a quantity of that electrical fluid, which they all in a natural ſtate contain. Were we to indulge conjecture, we might ſay, that by exciting a vibratory motion in the particles of ſolid bodies we produce a momentary change in their nature, in conſequence of which, that ſubſtance, which is the cauſe of heat and which they all contain, is capable while the motion laſts of producing a greater effect than it could do when the particles of the body were in a quieſcent ſtate. But were this the caſe, the moment the motion ceaſes, the body ſhould be as cold as it was at firſt. This is not ſo; the body preſerves its heat as long as on ordinary occaſions. May not this therefore be accounted for by ſuppoſing that friction does not leave the body after it ceaſes in the ſame ſtate as at firſt?

If the heat produced on mixing fluids depended on the friction which takes place in conſequence of the attraction which ſubſiſts between the particles, and which diſpoſes them to run towards each other with an accelerated motion, we ſhould expect that no change would be produced in the body, unleſs the friction was ſo violent as to alter the figure of

"*feu legerement bridé*," and when ſet looſe by the water added to it, he calls it "*feu pur et iſole.*" No experiment is brought in ſupport of this doctrine. The exiſtence of fire in this ſtate is not proved. By ſo large a portion of fire eſcaping, we ſhould expect great alterations in the qualities of the ſubſtances. Beſides, water ſeems to be but ill calculated for breaking theſe bonds which connect fire and the acid; for water, to do this, ſhould have a repulſion to fire: on the contrary, heat is attracted by water with great force. If this bridle were deſtroyed by the water, the greater its quantity the more heat ſhould be produced, which is not confirmed by experiment. Beſides, the ſame compound is produced by adding to an ounce of vitriolic acid eight ounces of ice, that is produced by adding eight ounces of water; yet the one produces cold, and the other heat. All the particles of fire which the acid contained when ice is uſed ſhould be retained in the mixture, while a part is diſſipated, when water is employed: this ſhould occaſion a difference in the qualities of the two compounds, which experiment does not ſupport.

This however may be accounted for in the manner of theſe chemiſts, and equally philoſophically, by ſaying that in ice there is a quantity of *froid bridé*, which deſtroys the *feu bridé* of the vitriolic acid. This is exactly ſimilar to thoſe authors who, to account for darkneſs, imagined there were certain

ſtars which rayed out darkneſs in the ſame manner as others ray out light.

The reaſon why heat is produced by mixing fluids ſeems to be, that the ingredients which compoſe the mixed fluid are in a different ſtate with regard to heat from what they were before. It would ſeem that whatever is the cauſe of heat, it has a greater effect in heating the bodies after the mixture than it ought to have had in proportion to its power of heating the ingredients before mixture. Two bodies, which being mixed have produced heat, are for ever after this much more eaſily heated and cooled, than we ſhould have been led to expect from the properties of the ingredients before mixture. Thus a mixture of acid of vitriol and water when ſet before a large fire, becomes as hot as the fire can make it in a leſs time than in proportion to the ingredients. Vitriolic acid in a phial, and an equal bulk of water in a ſimilar phial, being ſet before a fire, ſo that the fire ſhall act equally on them, the vitriolic acid will be heated as hot as the fire can make it in leſs than one half the time the water will be, though at laſt both will be equally hot. One of theſe phials filled with a mixture of equal parts of acid and water, and placed in the ſame circumſtances, ſhould require longer time to heat than the phial with acid, and leſs than that with water, and the time it takes ſhould be as much more than the acid took, as it is leſs than the water took. This by experiment

is not the caſe. The time which it takes is much leſs than in this proportion, and its heating or cooling is nearer the property of the acid than that of the water. From this it appears that when heat is applied to the acid and water after mixture, it either produces a greater effect on this than it did on the ſame matter when the bodies were ſeparate, or that the bodies when mixed attract heat with more force in the mixed ſtate, ſo that by running with more velocity, leſs time is required for the neceſſary quantity to go in.

The firſt of theſe is the caſe as appears from the following experiments: mix vitriolic acid and water in equal parts; call this the mixture. Take three veſſels of equal ſize and like ſhapes, and into the firſt put a quantity of vitriolic acid; into the ſecond an equal quantity of water; and into the third an equal quantity of the mixture. Let the three veſſels with their contents be heated to the ſame degree, ſo that all of them may be 100° hotter than a quantity of glaſs dry and in powder. Throw into each of theſe veſſels a quantity of the cold glaſs, till each becomes fifty degrees colder than it was, while the glaſs added becomes fifty degrees warmer. The quantity of glaſs neceſſary for this purpoſe will be proportional to the quantities of the cauſe of heat neceſſary to heat each of theſe bodies fifty degrees, or the ſame quantity of the ſubſtance, which is the cauſe of heat, applied to the ſame quantity of glaſs wil

produce the ſame effect, and a leſs quantity of heat will be neceſſary to heat a ſmall quantity of glaſs than a great one. Hence the cauſe of heat has a greater effect in producing heat when applied to the matter of vitriolic acid, than when applied to water in the proportion of the quantities of glaſs. For three pounds of glaſs have as much power in cooling a quantity of acid of vitriol as five pounds have on an equal quantity of water. But the effects which the cauſe of heat produced on the two bodies are equal; therefore the quantity of that cauſe is as the quantity of glaſs heated by the fifty degrees loſt by the water, to the quantity of glaſs heated by the fifty degrees which the vitriolic acid loſt, or as 5 : 3. If the acid and water, when mixed, were diſpoſed to be equally affected by the cauſe of heat as they were diſpoſed before mixture, then a mixture of equal parts of acid and water heated an hundred degrees hotter than the glaſs, and taken in the ſame quantity, and in the ſame veſſel, ſhould require four pounds of glaſs to cool it fifty degrees. It will be found by experiment that three pounds and a half will be ſufficient for this purpoſe; hence the mixture is more eaſily cooled fifty degrees than it ſhould be in proportion to its ingredients, and that in the ratio of 4 to $3\frac{1}{2}$, and the heat which is loſt had a leſs power of heating than it ought, becauſe theſe fifty degrees inſtead of heating four pounds of glaſs, as they ſhould have done, 50°, have only heated three and a half pounds to the ſame

point. Hence there is leſs of the cauſe of heat in the 50° which the mixture loſt, than there would have been had the mixture preſerved the qualities of the ingredients. And for the ſame reaſon that there is leſs of the cauſe of heat employed in the 50° than there ought to have been, there will be leſs of it employed in 500 or 5000 degrees. From this it ſeems reaſonable to conclude, that whatever is the cauſe of heat, it is capable of producing a greater effect in heating the mixture than it ought in proportion to the ingredients. The acid and water before mixture contain ſuch a quantity of the cauſe of heat as is ſufficient to produce all the heat in them from the loweſt degree of heat to the degree at which they are mixed. The moment that the mixture is made, the body is ſo changed, that the ſame quantity of the ſubſtance which is the cauſe of heat is capable of producing upon the ſame quantity of matter a greater effect than before, and this in the proportion of $3\frac{1}{2}$ to four, or as 7 : 8. Whatever effect therefore it would produce before mixture, it will after mixture produce one ſeventh more. Hence, if the two ſubſtances before they were mixed contained ſuch a quantity of the cauſe of heat as is ſufficient to produce when they are at 32° of Fahrenheit, an effect equal to ſeven times the difference between the heat of froſt and that of boiling water, or ſeven times 180°, after mixture, it will produce eight times this difference, or eight times 180°. The two ſubſtances

therefore, though as cold as froſt before mixture, will be as hot as boiling water after it.

If the proportions of acid and water be different from that of equality, the quantity of heat produced will be different. Thus one part of vitriolic acid mixed with eight parts of water becomes only 26° warmer, whereas from what happens when equal parts are mixed, a good deal more ought to have been produced. From which it ſhould follow, that the cauſe of heat does not produce an effect ſo much more in proportion to the ingredients, when the proportions are as 1 : 8, as when the acid and water are mixed in equal quantities. And this ſeems to be proved by mixing this compound of acid and water in the proportion of 1 to 8 with more acid; when we ſhall find that the whole, though as cold as the air before mixture, becomes exceedingly hot after it, but not ſo much ſo as if this quantity of acid had been mixed with an equal quantity of pure water. If the ſubſtance which is the cauſe of heat, inſtead of producing a greater effect in heating the mixture than it ought to have done, in proportion to its power of heating the ingredients before mixture, had not been able to heat it ſo much as it ought to have done, and this by an oppoſite change in the bodies; then, upon being mixed, the whole would have become colder than the ingredients were before mixture, and cold inſtead of heat would have been produced. Thus if the cauſe of heat, inſtead of pro-

ducing a greater effect in the mixture than it ought to have done in proportion to its power of heating the ingredients, and this in the ratio of 7 : 8, ſhould produce a leſs effect in the proportion of 8 : 7, then the cauſe of heat which was capable of heating the ingredients before mixture, from the loweſt degree of heat to that at which they were mixed, would, after the mixture, have produced leſs by one-eighth part, and one-eighth part of the heat in the body would diſappear upon mixture. If the two bodies, when mixed, were as hot as boiling water, and the whole effect of the cauſe of heat equal to eight times the interval between the freezing and the cooling points of water, the bodies, after mixture, ſhould immediately become as cold as *froſt*, or 180° of heat would diſappear from the body.

I do not at preſent recollect any inſtance of this happening in nature, where two fluids, on being mixed, become colder, unleſs perhaps the ſolution of certain ſalts in ſpirits of wine be of that kind. But we have many inſtances of it, when ſolids are diſſolved by fluids, as in the ſolution of ſalts in water, during which, except in one or two inſtances, the heat after mixture is leſs than before mixture. If the preceding account is well founded, this ſhould be owing to the cauſe of heat having a leſs effect on the ſame quantity of matter after mixture than it had on the bodies before. Something of this kind frequently happens independently of mixture,

when the ſame body is in a ſolid or in a fluid ſtate; as in ice and water, ſolid ſpermaceti, and melted ſpermaceti; lead in a ſolid and lead in a fluid ſtate, and the like, in all of which, the ſubſtance which is the cauſe of heat has a greater effect in producing heat upon the ſolid than it has upon an equal quantity of the correſponding fluid. If the ſolid becomes inſtantly fluid, the ſubſtance which is the cauſe of heat produces a leſs effect, and the fluid becomes colder than the ſolid body before this change took place. And in the ſame manner, if the body is fluid and is inſtantly made ſolid, the ſubſtance which is the cauſe of heat is capable of producing a greater effect than it did before, and the ſolid becomes hotter than the fluid was before the change of form was effected. This is confirmed by experiments on ice and water. When ice is mixed with an acid, the ice melts, and a compound of the ſame proportions is produced, as the ſame acid and water would have done. But the acid and ice become colder after than they were before mixture; the acid and water become hotter. This may be accounted for in the following manner:

The ſubſtance which is the cauſe of heat has a greater effect in producing heat in the mixture of acid and water than it ſhould have upon theſe bodies in a ſeparate ſtate, as has been frequently obſerved. But there is not ſo great a difference in this reſpect between the mixture, and the ingredients before

does on the water; whereas by experiment it produces on equal quantities of matter of the two bodies 28 times as much upon the mercury as it does on the water. In this respect it is very different from the ſubſtance which is the cauſe of gravity, which acts equally on equal quantities of matter. So that the gravity of the whole earth depends on the effect of this cauſe acting on every particle of it in proportion to its quantity of matter. In conſequence of which the power by which one heavy body attracts another, diminiſhes as the ſquares of the diſtance increaſe. How the cauſe of heat which affects equal quantities of matter differently, ſhould in a number of bodies equally heated have the power of heating bodies at a diſtance which diminiſhes in the ſame proportion, I do not know. I am equally ignorant whether the heat produced in thoſe mixtures, where bodies are inflamed and conſumed, and never capable of exhibiting the ſame appearances again, be owing to the cauſe which I have juſt aſſigned. It is very difficult to contrive experiments that will ſhew whether or not this is the caſe. There are even ſome experiments on the heat produced by mixing fluids that are not deſtroyed, which it is difficult to underſtand according to our theory. Whether theſe depend on ſome accidental circumſtances, or ſome other property of heat, muſt be left till experiments have furniſhed a ſufficient number of facts of this kind. Whether the ſub-

ſtance which is the cauſe of heat be a fluid of a ſubtle nature which pervades the denſeſt bodies of the ſame or a ſimilar kind with the electrical fluid, or that fluid which makes bodies cohere, or that which is the cauſe of animal ſenſation, is unknown. It might perhaps be poſſible to imagine a fluid endowed with ſuch properties, as by dint of a little mathematical reaſoning might be made to agree with all or moſt of the phenomena hitherto known. But then it is in the power of an accidental experiment to deſtroy the whole ſtructure we had been at ſuch pains to rear. It will be more uſeful to leave thoſe inquiries, and follow the method propoſed by Lord Verulam, the moſt certain in aſſigning the cauſes of the phenomena of nature. "*Non fingendum aut excogitandum ſed inveniendum eſt quid natura faciat aut ferat.*"

culation will be faſt or ſlow. But though in all animals the influence of the nerves on the heart and arteries is the principal cauſe of the motion of the fluids contained in theſe veſſels, yet this may be greatly effected by other cauſes, as heat or putrefaction. Every alteration in the conſiſtence of an animal fluid muſt affect its motion in the animal body. If the conſiſtence is diminiſhed, or the fluid rendered thinner, the ſame nervous influence will produce a quicker motion. If the fluid is by any means rendered of a thicker conſiſtence, every thing elſe being unaltered, this motion will be ſlower. The alteration in the conſiſtence of animal fluid depends chiefly on the degree of heat, though in ſome meaſure alſo on other circumſtances. If the heat is greatly diminiſhed, all the animal fluids will aſſume a ſolid form, in which caſe their motion muſt immediately ceaſe. If the heat is conſiderably increaſed, they either become too thin, and conſequently move with too much rapidity, or they undergo a change ſimilar to that produced by cold. Any great alteration of heat, therefore, muſt produce great alterations in the motion of the fluids contained in the vaſcular parts of an animal body. The blood, which in moſt animals is of a red colour, is the moſt important of all the fluids, and is of the greateſt conſequence, as from it all the other fluids of an animal are ſeparated, and how different ſoever they may appear in taſte or ſmell, or other qualities, either exiſted already completely formed

in the blood, or were produced from it by a ſmall alteration in the arrangement of its principles.

The blood of all animals that have been examined ſeems to divide itſelf into three principles. When it is allowed, after being ſeparated from the body of the animal to cool, undiſturbed in the open air, it reſolves itſelf into a watery liquor, and a thick liver-like ſubſtance of a red colour, which upon being waſhed becomes white, the colouring matter being ſeparated by the water. The blood cannot be again formed by uniting theſe ſubſtances together. The coagulum of the blood will not again diſſolve in the watery part, though it is heated to the ſame degree as the body of the animal.

The watery part of the blood, or, as it is generally called, the ſerum, upon being expoſed to a moderate degree of heat, conſiderably below that of boiling water, coagulates, or becomes ſolid, and never after can be made to aſſume a fluid form. If the ſerum is expoſed to a froſty air, it becomes ſolid at 25° of Fahrenheit. The blood of all animals, however, does not freeze at the ſame degree; for though that of the more perfect animals freezes at 25°, the blood of ſome of the inſect tribe remains fluid at 4°, or even lower than this. Therefore the blood of moſt animals can never ſuffer an alteration in heat equal to this, without being rendered totally unfit for circulating through the blood veſſels. And conſequently, no animal body can be heated thoroughly to 156°, and few cooled to 25°, without

We can form no idea of the degree of heat in our own bodies by our feelings. A perſon ſhivering, and under all the ſenſation of the bitterest cold, in a fit of the ague, is frequently found by the thermometer to be two or three degrees warmer than in health. Dr. Boerhaave imagined that the heat of hot animals approached very nearly to that at which the ſerum of the blood coagulates, and the great danger which attends a high fever he ſuppoſes to be owing to the riſk of the ſerum coagulating. But this, as I obſerved a little above, is far from being the caſe. For it requires a heat near 160° to coagulate the ſerum of the human blood, a temperature to which the body of no animal can endure to be raiſed. From this it is evident, that ſomething neceſſary to life is deſtroyed by a leſs degree of heat than what is capable of coagulating the blood. This is the nervous energy.

We find that we are ſcalded by water at a heat ſtill conſiderably below that at which the blood would coagulate. Animals differ more in this reſpect, than in the heat at which the ſerum becomes ſolid. A degree of heat which can be ſupported with eaſe for a conſiderable time by one animal will, in a few minutes, ſcald another to death. It is by means of this alteration in the nerves, that different animals bear different degrees of heat, and are ſo fitted for the different climates in this globe. Thus the birds, which inhabit our climate can, all of them uninjured, bear for a conſiderable time, a heat equal to 109°; while

ſome of the Greenland birds, upon being brought into the cabin of a ſhip, which may be about 46° or 48°, die in a few hours. The porpus again, whoſe blood is at 102° or 103°, can bear for ſome time water as hot as 96° without injury, while the ſame degree of heat would ſcald a perch to death in two minutes. Even the individuals of the ſame ſpecies differ in the degree of heat which they can bear without ſcalding. I have ſeen people put their hands with eaſe into water above 150, without being hurt by it, while it is intolerable to me at 114°. It is probable from many experiments which I have made, that perſons whoſe hands are not rendered callous by hard labour will find 114° ſcalding hot. The warmer animals of our climate can probably bear a greater degree of heat without pain than we can.

In general the nerves are incapable of being thoroughly heated, without injury, to any degree much above the heat of the blood. The heat can never probably in any diſeaſe be greater than 114°, and therefore an increaſe of heat never can produce death by coagulating the blood, but will, long before it arrives at the point neceſſary for this purpoſe, occaſion death by affecting the nerves, and depriving them of their ſenſibility and office.

The animals which feel cold to us have ſtill a power of generating a ſmall degree of heat, though greatly below that of the hot animals, ſeldom exceeding four or five degrees above the medium in

which they live. Of this kind are all the fiſhes, except the cetaceous, ſnails, frogs, and land and ſea tortoiſes, toads, vipers, and all the ſerpent kind, and moſt inſects. The frogs and tortoiſes have lungs as well as the hot animals, but they are of a very different texture, and the blood can eaſily circulate through their bodies, without paſſing through the lungs at all. The degree of heat produced in theſe animals is not the ſame in all. The leſs perfect the animal, the lower the degree of heat. Hence ſhell-fiſh, as oyſters, muſcles, &c. have ſcarcely any perceptible heat above the ſurrounding medium. Fiſhes of a more perfect kind, as the perch, have but a ſmall degree of heat, about one degree, above the medium in which they live. This heat ſeems to be at the ſame degree above the medium in which they are, of whatever temperature that may be. Thus the flounder, cod fiſh, and haddock, were not found above a degree warmer than the ſalt water in which they were ſwimming, even when that was ſo low as 41°; trouts were at 62°, when the water they were ſwimming in was 61°. Snails have a heat about 2° higher than the air. Frogs and land tortoiſes have a power of generating heat ſomething ſtronger, about 5° higher than the air in which they breathe. This is likewiſe the caſe with ſea tortoiſes, toads, vipers, and all the ſerpent kind. All the inſect tribe as far as is hitherto known, excepting bees, belong to the cold animals. Caterpillars are but a degree or two above the air they live in. Bees

belong to the hot animals, and a thermometer placed in a bechive ſtands at 97°, the human heat.

By far the greateſt part of cold animals live in water, which they can very well do without having any great power of generating heat, as water is never colder than 32°, and ſeldom acquires a heat above 60° or 70°. Hence they ſtand in leſs need of a ſtrong power of generating heat, than the animals that live in the air, which is liable to a much greater alteration of temperature than water is. The quantity of heat generated by cold animals is not very great on ordinary occaſions.

But in ſome particular caſes, when the heat of the medium in which they live is in danger of being diminiſhed below a certain pitch, then the quantity of heat generated in a given time is much greater. The following experiment proves this. Two phials of the ſame ſhape and ſize with an equal quantity of water in each were immerſed in a mixture of ſnow and ſalt in a room whoſe temperature was 24°. Into one of them was put a ſmall fiſh; the other ſerved as a ſtandard. The phial in which there was nothing but water froze in a little time. The phial with the fiſh ſoon had a ring of ice formed at the ſides of the phial, but a quantity of water immediately in contact with the fiſh could not be frozen in leſs than half an hour after the water in the other phial was frozen, though the quantity of the mixture of ſnow and ſalt was very conſiderable. The heat generated by this little animal during this time

muſt have been very great, to have reſiſted the freezing power of ſo large a mixture. After all, the water could not be frozen, till the fiſh, by the violence of its ſtruggles, in conſequence of its tail being fixed in the ice, had killed itſelf. The heat of this fiſh was about a degree above the heat of the water at 34° in which it was ſwimming.

All the cold animals are ſcalded to death with a degree of heat, which would not hurt the hot animals. But moſt of them can bear a much greater degree of cold without injury. Fiſhes, as they breathe water as we do air, cannot live when the water freezes by the cold. All the other cold animals bear the winter without injury. But they generally retire into lurking places, ſo as to be affected with it as little as poſſible. The inſect tribe, though the moſt tender and delicate of all animals, can bear, unhurt, the greateſt degree of cold. They are preſerved in the coldeſt ſeaſons, with little other ſhelter than the leaves, and the rugged barks of trees and ſhrubs, holes in walls, or a very ſlight covering of earth; and ſome of them expoſe themſelves perfectly undefended. Dr. Boerhaave obſerves that in the bitter winters of 1709 and 1729, inſect eggs and inſects, in the ſtate of nymphæ, outlived the cold, that was intolerable to the livelier animals. Even in the coldeſt climate of this globe, there are innumerable ſwarms of flies, whoſe eggs and aureliæ muſt have ſurvived the winter cold. The French mathematicians in Lapland were greatly peſtered

with theſe animals, though the cold of their winter was 33° below the beginning of Fahrenheit's ſcale. Reaumur obſerves, that very young caterpillars bear without injury a degree of cold equal to 4°.

The motion of the heart of the cold animals is much ſlower than that of the hot animals, and during the ſevere cold of the winter it does not move at all. Dr. Harvey has ſeveral facts to this purpoſe. In ſnails, the heart, which beats very manifeſtly in ſummer, was found to be perfectly at reſt in winter. The ſame fact appeared in many of the fly tribe and others, which paſs the winter in a ſleepy or inſenſible ſtate. Theſe animals ſeem to have a kind of life more of a vegetable than animal nature. Of this ſort are the viper and ſerpent tribe, frogs, toads, and tortoiſes; even the bat, which is naturally a hot animal, paſſes the winter in this ſtate of ſleep or inſenſibility, and during this period is as cold as the ſurrounding medium, and its heart is perfectly at reſt. Yet theſe animals, in this ſituation, ſcarcely to be diſtinguiſhed from death, are immediately reſtored to their original ſtate of activity and warmth, by being kept for ſome time in a warm place.

Though the warm animals never have their blood much warmer than its ordinary temperature in health, and are incapable of bearing ſuch increaſed heat, they can, however, bear degrees of cold far below it without danger. Boyle gives many inſtances of animals that have become ſtiff with cold, or which have been buried in lumps of ice, that by being expoſed to a warmer

air, have been again reſtored to their life and ſenſes. Partial freezing of the noſe, hands, and feet of the human body are frequent. Hildanus gives an inſtance of men ſo frozen by the maſs, that when plunged into cold water they contracted a cover of ice over the whole body, and therefore muſt have been of a temperature conſiderably below 32. Yet even in this terrible ſtate people have been recovered to life again by a method of treatment well underſtood in theſe intemperate climates. The great degrees of cold, which the different parts of the human body frequently ſuffer, are equal to, or even below what is neceſſary for freezing the blood. It may, therefore be imagined, that in this caſe it would be impoſſible to reſtore the body to its original ſtate, eſpecially, as it was aſſerted in a former part of this eſſay, that if the blood was once coagulated, it was impoſſible by expoſing it to heat, ever to make it regain its original fluidity. But though this is the caſe upon expoſing blood to the open air to cool ſlowly and gradually, and has hitherto been ſuppoſed to happen in every caſe, yet if the blood is expoſed to a great degree of cold, and ſuddenly cooled, ſomething different happens. If the blood is cooled to a great degree, and ſuddenly frozen, it can be preſerved in that ſtate for any length of time without becoming putrid, and upon expoſing it to a gradually increaſing heat, it becomes as fluid as when it ran from the vein of the animal; and after this, if it is allowed to cool ſlowly in the

open air, it will coagulate like blood freſh drawn, and then cannot be again reſtored to its natural fluidity by heat. Now in all theſe partial freezings of the human body, the blood muſt be frozen before it can coagulate; for the cold is too ſuddenly applied to give it time to coagulate before it freezes, and therefore the blood in the veſſels regains its original fluidity, when the frozen member is reſtored to warmth.

It appears from theſe obſervations, that though the nervous influence is deſtroyed by much leſs heat than what is neceſſary for coagulating the blood, that it ſuffers very little by a degree of cold, which eaſily freezes that fluid. For in every caſe where a frozen member of the human body has been reſtored to its natural ſtate, it is evident that the nervous energy remains, otherwiſe the reſtoring the fluidity of the blood would be of no conſequence; the part would infallibly mortify; for the blood veſſels, in this caſe, deprived of their ſenſibility, would allow the blood to ſtagnate for ever in them. However, though the nervous influence can be reſtored, when the nerves have ſuffered a degree of cold ſomething below that of freezing blood, yet if the cold is much below this, they are entirely deſtroyed, and cannot be again reſtored to their ſenſibility at all. Hence in many caſes frozen hands and feet become gangrenous in ſpite of every method of treatment. When a member has been froſt-bit, it is neceſſary to reſtore the heat to it very gradually. If it is put

immediately into warm water, it infallibly mortifies. Hence in cold climates, where accidents of this kind are frequent, they put the frozen limb into a mixture of ſnow and water, or rub it with ſnow for a conſiderable time. All ſudden alterations of heat are dangerous to the bodies of animals. The difference between the heat of the frozen member and that of water, which we could call hot, is 70 or 80 degrees; and the ſtate of the nerves at that time would probably make water at 70° or 80° produce the ſame effect as ſcalding water would have on them on ordinary occaſions.

The animals which paſs the winter in a ſtate of inſenſibility are ſuch, probably, whoſe blood is frozen with a much leſs degree of cold, than that neceſſary for deſtroying the nerves. Hence they paſs the winter without requiring nouriſhment, and without any waſte of ſubſtance or danger of putrefaction. When the returning ſun in the ſpring reſtores the life of all the vegetable kingdom, the blood of theſe ſleeping animals regains its fluidity; the animal is reſtored to its ſtate of activity and vigour, in the ſame manner as a froſt-bit limb is reſtored to life by a gradual increaſe of heat. If the winter has been very ſevere, the cold may be ſo great as to deſtroy their nervous influence altogether, ſo that the animals cannot be reſtored to life by the returning warmth. In order to prevent accidents of this kind, they retire into holes in the earth, where they are in leſs danger of ſuffering too great a degree

of cold. Hence, if an animal, whoſe blood froze before its nerves were deſtroyed, were expoſed to a ſudden cold, it might be reduced artificially to this ſtate of inſenſibility, like the animals, which paſs the winter in a ſtate of inaction, and might be kept for any time in this ſtate, and reſtored to life whenever we pleaſed, by a gradual application of heat. I once aſſiſted at a trial of this kind on a fiſh; but we could not freeze it ſuddenly, on account of the power of generating heat, which the animal was found to have, as already mentioned, ſo that it was killed by its own ſtruggles before it could be frozen. A ſwallow would be more proper for this trial. Every body knows, that flies, which are apparently dead after a ſevere froſt, will recover in the ſun beams, after being in this ſtate of inſenſibility for ſeveral days.

This account of the matter, and theſe experiments, render this opinion about animals that die in winter very probable. Future experiments, however, will throw more light on this ſubject.

others at leaſt with regard to its general qualities. The vapour of all bodies is elaſtic, and its elaſticity is inverſely as the ſpace which it occupies.

The production of vapour from fluids is attended ſometimes with a particular commotion, which cannot take place in ſolids. This boiling in fluids may be produced at very different degrees in the ſcale of heat. Theſe variations are found to depend on the ſituation of the body with regard to the preſſure of the atmoſphere. If the preſſure is increaſed, the heat neceſſary to make the fluid boil is greater, and the preſſure being diminiſhed, is accompanied with a diminution of the boiling heat. Theſe circumſtances have been long eſtabliſhed by the moſt undiſputed experiments, and it is now univerſally agreed that the preſſure of the atmoſphere, and the boiling heat of fluids, are in a conſtant ratio to each other, and one of them being given, the other may be immediately diſcovered.

Evaporation has been divided into ſenſible and ſpontaneous. But theſe are the ſame, differing only in degree. It is not ſolely at the boiling point that fluids are converted into vapour. They alſo evaporate in a degree of heat far below it. But the quantity of vapour produced from a body in a given time, is greater when it does, than when it does not boil. The vapour is alſo in the firſt caſe very elaſtic and difficult to be confined excepting in veſſels of great ſtrength; whereas the vapour produced below the boiling point has but little elaſticity, and may

eaſily be confined in the weakeſt veſſels. Philoſophers have not yet diſcovered the leaſt degree of heat by which a body may be converted into vapour. We find in the caſe of water that it is converted into vapour even in the form of ice without paſſing into a fluid ſtate. If we breathe on a poliſhed plate of metal that has been expoſed to the air during an intenſe froſt, a film of hoar froſt immediately congeals upon it; but in a ſhort time this diſappears, though the air be much colder than froſt, and without any drops of water being ſeen upon its ſurface. But evaporation in this caſe proceeds very ſlowly, in ſome meaſure depends on accidental circumſtances, and varies at different times while the external temperature is the ſame. The dryneſs or moiſture of the air ſeems to have conſiderable effect upon it.

As water forms a very great part of this earth, we have opportunities of making more obſervations on the evaporation of that fluid. We ſhall therefore confine our remarks chiefly to it. From the obſervations, which every body has made, this fluid appears to evaporate more quickly the greater the degree of heat to which it is expoſed. We perceive that evaporation is greater in hot than in cold weather, that artificial heat produces vapour in ſtill greater quantity, that the more we increaſe the heat the more quickly the water evaporates. Theſe things are ſo obvious that one might have omitted them, were it not that a very ingenious and cele-

brated writer has lately diſputed them, and advanced an opinion that ought not to be paſſed over without examination. The author I mean is Mr. Bergman, chemical profeſſor at Upſal. This gentleman, in a late volume of his *Opuſcula* ſays, that almoſt every operation in chemiſtry has a certain degree of heat, as it were, allotted for it, and in which it may be performed in a leſs time than by either a greater or leſſer degree of heat; that water is ſubject to this rule; that it is converted moſt readily into vapour at 150° of the Swediſh thermometer, which is equal to 302° of Fahrenheit; that this point in the ſcale of heat may, with regard to water, be conſidered as ſimilar to a point of contrary flexure in a curve on each ſide of which the ordinates diminiſh gradually till they vaniſh.

According to this doctrine the hotter the fire is above 302, the leſs water is diſſipated, and we are led to conclude that water intenſely hot is not more diſpoſed to become vapour than in the ſevereſt froſt, and that heating water red-hot in a piſtol-barrel, which has hitherto been reckoned a hazardous experiment, and which was thought no ſmall proof of courage in the gentlemen who firſt tried it, is, on the contrary, very ſafe; at leaſt the great danger is at 302°, but beyond this it gradually diminiſhes, and at the red heat at which the piſtol burſt, it ſhould have vaniſhed altogether. I wiſh Mr. Bergman had mentioned the experiment on which he founds this opinion. On moſt other occaſions he has not taken

up opinions raſhly, but has proceeded with great caution and attention, and appears to have perfectly underſtood the principles of the Baconian philoſophy. In the preſent inſtance, however, his opinion ſeems to be ſo contrary to common ſenſe and obſervation, that few will aſſent to it, but moſt perſons be apt to ſay that it deſerves no ſerious conſideration. It is ſimilar to that notion which has been lately ſtated to prove that none of the ſtars are much larger than our ſun. It is affirmed that if one of the ſtars was five hundred or a thouſand times larger than the ſun, and placed at the ordinary diſtance we commonly ſuppoſe the fixed ſtars to be, we could not ſee it, becauſe before its light could reach us the whole of it would have fallen back to the original body by the force of attraction. We muſt not, therefore, expect to ſee a luminous body merely becauſe it is large, for beyond a certain ſize, the larger it is the leſs probability we have of ſeeing it. Our only chance is, that it ſhould be ſmall.

It is not eaſy to find out in what manner Mr. Bergman could prove by experiment, that water evaporated faſter at 302° than at any other degree. For water flies off in vapour long before it can be heated to this point in the open air. If we apply artificial preſſure to the ſurface of water, it may acquire this degree of heat, but it does not evaporate. If we apply preſſure till the water heats to 302°, and then remove the preſſure, the water evaporates in part, but more of it, I find, if the preſſure is continued

till the heat be raiſed to 500. Theſe things are oppoſite to Mr. Bergman's notion, who in this caſe ſeems to inſinuate that heat is not the cauſe of evaporation. All experiments prove this to be without foundation, and we may ſtill admit, that the greater the degree of heat to which water is expoſed, the more quickly it will evaporate; and I believe we may alſo admit that the larger a body is, the more eaſily it will be ſeen.

Water when converted into vapour increaſes in its ſize about 1800 times, but this is ſtrictly applicable only to vapour produced at the boiling point. It is not eaſy to aſcertain the expanſion of water when converted into vapour in lower temperatures, though it is probably not much leſs than this. Many authors, among whom is Deſaguilier, make the expanſion above 14000 times. But the experiment from which he has taken this number does not admit of an expanſion much above the tenth part of it. The vapour of water is therefore much lighter than air, which is only between 800 and 850 times lighter than water. The ſpecific gravity of ſteam to that of air is as 1800 to 800 or as 9 : 4.

If vapour could retain its form independent of external cold, it would riſe till the denſity of the air was diminiſhed in the proportion of 9 : 4. At this place the barometer would ſtand at 13.33 inches, and the perpendicular height above the level of the ſea would be ſomething leſs than four miles.

But as the vapour riſes, the preſſure on it would

diminiſh, and at the height of four miles being preſſed with but one-half of the force, it would occupy double the ſpace it did at the ſurface of the earth. It would ſtill, therefore, continue to riſe; and the ſame cauſe continuing to operate, it would reach the top of the atmoſphere.

The whole vapour, however, never reaches to the height even of four miles perpendicular. A conſiderable part of it is condenſed before it can riſe to that height, and the part which does not condenſe is no longer pure vapour, ſuch as we have ſuppoſed it at firſt. Though a great part of the vapour of water is condenſed before it riſes four miles perpendicular, a part of it aſcends near as high before it condenſes. This is evident from the ſnow on the tops of hills. The ſummit of the Andes is covered with ſnow though under the equator, and above a certain height the ſnow conſtantly lies. This height the French academicians call the level of the ſnow.

The vapour of water then does not always return immediately to the ſtate of water upon being cooled. A part of it mixes with the air, and is diſſolved by it, and may then riſe to a great height in the atmoſphere. When we ſtand near the boiler of a fire-engine, and the machine ſtops, and the ſteam is let out, a white cloud of vapour iſſues with great violence, but after riſing a little way it diſappears. It does not, however, immediately fall down again in the form of water, though from the temperature

of the external air it muſt inſtantly be cooled, far below the boiling point. This muſt be owing to the vapour being diſſolved in the air. For, if we hold any cold body in this ſteam, and retard its mixture with the air, drops of water are inſtantly formed on it. When hot water is put under the receiver of an air-pump, and the air taken out, the water evaporates, but this vapour immediately condenſes upon cooling, and little or none will be found in the receiver, when it has acquired the temperature of the external air. The air at all times contains a quantity of vapour diſſolved, and may be made to part with it, either by cold, or by expoſing bodies to it which have a ſtrong attraction for water, and which will ſoon lay hold of the vapour, unite with it, and be diſſolved by it. Air, which has diſſolved vapour, is lighter than pure air. Equal parts of air and vapour ſhould produce a compound, whoſe denſity to common air ſhould be as 8 : 13. Suppoſe the air to be free from all vapour of water, when the barometer ſtands at 30 inches, if at any time the air was mixed with an equal quantity of watery vapour, the barometer ſhould fall to 19 inches. In our climate, the barometer ſeldom falls below 28 inches; which, independent of all effects from accidental circumſtances, of rarefaction from wind and other cauſes, would give the proportion of vapour mixed with air, when greateſt in our climate, to amount to a tenth part. In this caſe, a gallon of air ſhould be found to contain 3.64 grains of

water, about three and a half grains. This, I believe, is less than may be sometimes separated from it, both by cooling it, and other means. In a mixture of equal parts of air and vapour, the gallon should be found to contain from 16 to 17 grains of water. I have here supposed the specific gravity of air to be $\frac{1}{800}$ of that of water, and the heat, the mean heat of this earth, to wit, 56°.

I have taken it for granted that air in some cases has no vapour mixed with it; this, however, never happens. I have also assumed, that the vapour and air after mixture occupy the same space which they did before mixture, which is also not exactly true. These will affect the conclusions a little, but not so much as might at first sight be imagined. The rising and falling of the barometer depends more commonly on the quantity of vapour mixed with the air, than upon winds or any other causes that have been assigned.

When vapour is produced suddenly from a fluid it becomes colder. It has long been observed in China, that wet cloths exposed to the air are colder than the temperature of the air, as long as they retain any moisture. Accordingly, in that country, rooms and galleries formed of canvas are kept constantly wet to give coolness to the air within. They also cool jars containing water in the same manner, and upon this principle also, they are enabled to make ice in places where the temperature of the air is seldom so low as the freezing point. Though

the Chineſe ſeem to have firſt obſerved that the evaporation of water produced cold, they do not ſeem to have remarked it in other fluids. It is, however, now known, that all fluids when they evaporate produce cold, and that the cold produced is in proportion to the volatility of the fluid. By moiſtening bodies with ether, we can cool them below the freezing point in the hotteſt day in ſummer. When water is expoſed to an intenſe heat in a cloſe veſſel, it becomes much colder immediately upon opening it, and allowing the vapour to iſſue. But this vapour is not hotter than the common boiling point of water. The quantity of heat which diſappears in this caſe would have heated the fluid 800°, as I have found by experiment ſome years ago. One may naturally aſk, is the production of cold the conſequence or the cauſe of the evaporation? But we know too little about vapour to be able to give a ſatisfactory anſwer to this queſtion. It may be owing to the one or both, or to ſome other cauſe unknown to us. The only thing we are ſure of is, that the production of cold, and the converſion of a body into vapour, always go together. Another circumſtance in vapour is, that it continues ever after to be more difficultly heated than in the ſtate of water, and from this, the cold produced in evaporation may be accounted for, as I mentioned in a former eſſay.

Great alterations in the heat of the weather may be expected to ariſe from the cauſes juſt ſtated. For

vapour is always forming or condenſing, and according as the one or other prevails, the heat produced exceeds the cold, and the contrary. The quantity of vapour formed by the ordinary heat of the weather far exceeds that produced by artificial fire. It has been found that the water which evaporates from an acre of new mown meadow in twelve hours amounts to ſixteen hundred or two thouſand gallons, and when the ſun's rays acted with great force, by being reflected from a brick wall, two thouſand eight hundred gallons were evaporated from an acre. In the torrid zone it is not improbable, that during the twenty-four hours in the rainy ſeaſon, the water evaporated from an acre will be double this quantity, or will amount to five or ſix thouſand gallons. To this muſt be added the great quantity of ſmoke which ariſes from the inflammation of combuſtible bodies and which conſiſts chiefly of water.

The cauſe of the formation of vapour has been variouſly aſſigned. Philoſophers have been much intereſted in the queſtion, but none of the theories are quite ſatisfactory. It is not my intention to mention them all. Two have been reckoned more probable than the reſt, and deſerve on that account to be noticed. The firſt is, that water is converted into vapour by being diſſolved in the air, in the ſame manner as a ſalt diſſolves in water. The other opinion is, that the water is converted into vapour ſolely by heat. The firſt of theſe is apparently well ſupported by many circumſtances. Such are,

that water evaporates faſter in hot than in cold weather, becauſe hot air diſſolves more water than cold air, and that air, which is very warm, upon being cooled, depoſits a part of the moiſture which it formerly had kept ſuſpended; that a ſtream of air diſſolves water, or evaporates faſter than air that is at reſt, in the ſame manner as agitation of water, though it does not indeed increaſe the quantity of ſalt diſſolved, makes the ſolution go on faſter. According to this theory, the evaporation ſhould always be in proportion to the heat of the air, and ſhould depend very little, or not at all, on the heat of the water to be evaporated. But hot water evaporates faſter though the air be cold, and the quantity evaporated varies with the heat of the water, while the temperature of the atmoſphere remains unaltered. But beſides this, there is another objection, which is, that from this theory, the water ſhould not evaporate in vacuo. But water, by experiment, does evaporate in vacuo, and this, not only when the heat is near the boiling point, but alſo in the ordinary temperature of the weather. We muſt, therefore, ſeek for ſome other cauſe of evaporation. It has been indeed commonly aſſerted, that water evaporates faſter in vacuo than in the open air; but this has not been ſupported by experiment, at leaſt, in the ordinary temperature of the weather.

The other opinion is, that water is converted into vapour by means of heat. That water may be vaporiſed independent of air, is evident from the

experiments in the air-pump, where water boils with violence and is converted into vapour, whenever the heat is greater than 100° of Fahrenheit; water alſo put in the top of a common barometer, where the vacuum is ſtill more perfect than in the air-pump, evaporates very readily. It is evident, therefore, that air is not the ſole cauſe of evaporation; that it often goes on as faſt or even faſter without it, if the heat be above 100° Fahr. The cold produced by evaporation in the ordinary heat of the weather is exactly proportional to the quantity of vapour produced, and follows the ſame rule as at the boiling point in the open air. For theſe reaſons we muſt admit that heat is the primary cauſe of evaporation, and without this it cannot go on. But ſtill ſome difficulties remain. Why ſhould evaporation proceed faſter in the open air than in vacuo, even in cold weather? Why ſhould the wind greatly promote evaporation, and certain winds more than others? And laſtly, why ſhould the vapour riſe ſo high without always falling down in rain or ſnow? If heat was the ſole cauſe of ſpontaneous vapour, the interval between the evaporation of the water and its falling down again could not be ſo great, nor could the vapour get time to collect in ſufficient quantity to form theſe long continued rains which take place in ſome countries. Neither can we underſtand why there ſhould be ſuch intervals of dry weather: when theſe circumſtances are conſidered, it is not poſſible to admit, that heat alone

is the cauſe of ſpontaneous vapour. Evaporation ſeems to be the joint effect of both heat and air. There ſeems to be two ways in which this may be effected; either by the air acting on the water in its ordinary form, and diſſolving it immediately, or by the air combining with the vapour of the water, and preventing it from condenſing and falling down in rain ſo faſt as it would otherwiſe have done. The vapour which heat raiſes is inſtantly united with the air, and by that means prevented from condenſing. The quantity of vapour which the air is capable of diſſolving depends on two circumſtances, its heat, and the moiſture already combined with it. If it is warm, every thing elſe being equal, it will unite with more vapour than when cold. But warm air may already be ſaturated with vapour: it will then take up no more, and the vapour then raiſed by heat will condenſe again into water. If the air is very cold, it may ſometimes be capable of combining with more vapour than air apparently warmer, becauſe air, when cooled, neceſſarily parts with ſome of the vapour it had diſſolved. If, after this, it ſhould be ſuddenly heated, though but for a few degrees, it will diſſolve vapour very faſt: for the cold to which it had lately been expoſed, having thrown out of it a great deal of vapour, it is, though ſtill very cold, diſpoſed to attract it very greedily, and much more quickly than hotter air loaded with vapour. The vapour in our breath is viſible in a cold day, becauſe the air is not then able to diſſolve

it; but it is not viſible in a hot day, unleſs the air be already ſaturated without vapour, which it ſometimes is.

It has been found by experiment, that removing the vapour which ſtagnates over a hot fluid increaſes the evaporation, even when the quantity of vapour is ſo great that the air has not time to diſſolve it. A ſtream of air, though ſaturated with vapour, may in ſome caſes promote evaporations, and the drying effect even of moiſt winds may probably depend upon this. Perhaps alſo air diſſolves water, or even ice, in their ordinary form, though this is not ſo clearly proved by experiment, and moſt of the phenomena of evaporation may be explained without it. At the ſame time, we have inſtances of bodies differing almoſt as much in denſity as air and water, which diſſolve each other, and the principal difficulty lies in conceiving, how air, by diſſolving a denſe body like water, ſhould form a compound ſpecifically lighter than it is when pure. If this be really the caſe, it ſeems to be neceſſary to admit, that water is converted into vapour previouſly to its combining with the air, and, if I remember right, ice in vacuo expoſed during an intenſe froſt, actually diminiſhed in weight, and evaporated. This is not ſo ſurpriſing, when we conſider that mercury, whoſe vaporific point is 672°, evaporates in vacuo below 212 from Colonel Roy's experiments. If the vaporific point of ice in vacuo be as far below the boiling point of water in the open air, ice

ſhould evaporate in vacuo at 24° below the zero of Fahrenheit.

Moſt authors ſeem to believe, that air combined with vapour is heavier than dry air. But if this were the caſe, why does it riſe ſo faſt through the reſt of the air. It is not eaſily conceived that this is produced by the chemical attraction between dry air and air loaded with vapour, for the aſcent of the moiſt air ſhould in this caſe be ſlow. Neither will wind nor agitation of the air account for it; for vapour riſes when there is no wind. Air that has been heated through the day, and which cools ſoon after ſunſet, depoſits in the form of dew, ſome of the vapour which it cannot then keep diſſolved; though ſometimes dew is produced by the vapour iſſuing from plants, &c. which is condenſed from the air being too cold to diſſolve it. When the air near the ſurface of the earth has diſſolved vapour, and a cold wind immediately riſes, the air containing the vapour is cooled, the further ſolution of vapour is prevented, a part of that which was diſſolved ſeparates, and an opacity in the air is produced from the watery vapour condenſing, and a fog or miſt ariſes. Sometimes this happens only in the morning, and ſoon after ſun-riſe the air being heated re-diſſolves the vapour, and the day clears up.

The air conſiderably higher than the ſurface of the earth, is colder than that below. When warm air which has diſſolved vapour riſes to a certain height, it will cool, and depoſit ſome of the vapour

which it had diſſolved, and this will appear in the form of clouds. The height at which this happens, depends on the heat of the weather, and hence the clouds are generally higher in ſummer than in winter, in a hot than in a cold day. Hence hills, the tops of which are colder than the valleys, and nearer to the place where the ſeparation of vapour from the air by cold takes place, attract a great deal of the vapour which condenſes; and hence the moiſture on hills, and the ſprings of water that are there to be met with. If the mountain is very high, the air will have parted with moſt of its vapour before it reaches the top, and hence the ſummit of very high hills is often clear, when there are clouds on the lower parts. It is, perhaps, alſo owing to this, that, on the tops of very high mountains, the air is very dry, as De Luc obſerved it to be on ſome of thoſe in the neighbourhood of Geneva.

A conſiderable part of the vapour diſſolved in the air would ſeparate as it riſes, independent of cold. When we rarefy the air in the receiver of an air-pump, the operation is always attended with a miſtineſs and opacity in it, which is owing to the vapour being incapable of rarefaction beyond a certain degree, without a part of it condenſing and furniſhing heat to the increaſed capacity of the remainder; otherwiſe it could not exiſt in this rarefied ſtate, as I have endeavoured to ſhew in a following eſſay, on the reaſon why the falling of the barometer is generally attended with ſnow or rain.

In a few inſtances, vapour of water, and even of other bodies, may be found ſtill higher than the tops of the higheſt mountains; and meteors, if they are not ſolid bodies, are either produced from vapour riſing to a prodigious height in the atmoſphere, or are a conſequence of that vapour. We do not ſufficiently underſtand the nature of theſe bodies, but from the attention that is for the future likely to be beſtowed upon them, we have reaſon to hope that we ſhall ſoon become better acquainted with them. The vapour ariſing from ſome ſubſtances will not combine at all, or only in a ſmall quantity, with the air. The ſubſtance known by the name of inflammable air is of this kind, and as it is much rarer than common air, it may riſe to a very great height in the atmoſphere. We do not yet know exactly the height to which meteors riſe, though the laſt one, it was imagined, was not very high. If theſe bodies are really produced by electricity very high in the air, this may ariſe from the vapour of bodies getting into a different ſtate, and condenſing into water or ſnow. By ſome late experiments, it has been found, that water converted into vapour by a fire whoſe electrical communication is cut off by ſetting it on a glaſs plate that was cooled on one ſide, gives an electrical ſpark every time water is thrown upon the coals. The vapour that iſſues muſt therefore, in its natural ſtate, contain either leſs or more electricity than the water from which it was produced, and after riſing to ſuch a great

height as to be out of the reach of any conducting ſubſtance, a part of it may very ſuddenly condenſe into water; the whole would become charged with electricity either poſitive or negative. The electricity in the clouds, it is not improbable, may ariſe from vapour condenſing into rain, or freezing into ſnow or hail, and the clouds being plus or minus, may depend on the condenſation being either from vapour into rain, or vapour into ſnow. We have reaſon to expect electrical appearances wherever the vapour of bodies reaches, and this electricity ariſing from a partial condenſation of vapour or ſeparation of it from the air, may greatly affect the part of the vapour which ſtill remains combined with air, and diſpoſe a part of it to ſeparate ſooner, and the reſt to riſe to a greater height than it otherwiſe would have done. It ſeems to be alſo extremely probable, that the aſcent of vapour and its diſpoſition to ſeparate from the air, depend much on the ſtate of the atmoſphere with regard to electricity. But this I ſhall not at preſent diſcuſs.

ESSAY IV.

ON RAIN.

THE greateſt part of the water which is evaporated from the ſurface of this earth by the heat of the ſun, deſcends again in rain. The reſt falls in the form of ſnow, hail, and dew. Rain, ſnow, and hail fall in great quantities at one time, and not at all in another; but there are few days in which there is no dew. The quantity of rain greatly exceeds, at leaſt in temperate climates, the quantity of ſnow and hail, and may, therefore, be conſidered as the principal cauſe of that conſtant deſcent of water in fountains and rivers from the higher to the lower parts of this globe, upon which the exiſtence of vegetables depends, and by whoſe means this earth becomes a healthful as well as delightful habitation to man.

The quantity of rain is different at different ſeaſons of the year, in different climates, and in different

ſituations in the ſame climate. More falls near high mountains than in the ſame latitude in a level country. Many inſtruments have been contrived, by which the quantity of rain may be meaſured with tolerable accuracy. By means of obſervations made with theſe inſtruments, it has been found, that the quantity of rain in one place is often double that at another, though the diſtance between them be but ſmall. The quantity of rain in ſome parts of Lancaſhire is nearly double what falls in London. The quantity of rain in London and Paris is nearly the ſame, though the diſtance of thoſe places is not leſs than that between Rippon in Lancaſhire and London. The quantity of rain at Piſa in Italy, a place ſituated 40° 37′ W. or 7° South of London, is much greater than at London or Paris, and equals that in Lancaſhire. This is the more ſurpriſing, when we conſider, that Piſa is but five miles from the ſea. It is in general agreed, that there is more rain in the temperate than the torrid zone, more in a high than a low latitude. In certain ſituations, however, this does not take place. The neighbourhood of high land will make a place much more rainy than it otherwiſe would have been. Every perſon has obſerved that it frequently rains on the tops of hills, while it is fair in the low country. When there is more rain in a place near the equator than in another at a conſiderable diſtance from it, we generally find that high land near the moſt equatorial place is the cauſe of the exception to the general rule. Though,

however, the quantity of rain near the equator may, from neighbouring high land, equal or even exceed that in places near the poles, yet the number of rainy days is greater in the temperate than in the torrid zone; and greater in thoſe parts of the temperate zone which are fartheſt from the equator. In many places near the equator, the quantity of rain is as great as any where in Britain, but the whole of this rain falls in a few days, whereas with us, it rains 70 or 80 days of the year at a medium. This difference in the time, in which a given quantity of rain falls, will occaſion a great difference in its effects upon the ſoil of the two countries. In the one country, the rain falls in torrents. The greateſt part of it runs with violence along the ſurface of the ground, deſtroying vegetables and animals, carrying the ſoil along with it, and but little gets time to ſink into the ground, ſo as to ſerve for the refreſhment and growth of vegetables. On the contrary, where rain falls gradually, a great part of it penetrates the ſoil, and ſerves as nouriſhment for vegetables; the reſt appears again at a conſiderable diſtance in the form of ſprings and fountains.

We cannot judge of the moiſture of a country by being barely told how much rain falls in a year. A country may be very dry, though more rain falls in it in a year than in any part of Great Britain; yet the whole of this iſland is to be reckoned moiſt. Were it to rain with the violence it does in ſome places near the equator, 20 or 22 inches of rain

might fall in ten days. But though this quantity of rain be conſiderable, if there be no more, there would be 355 dry days and 10 wet ones that year; the air would be in general dry, and the climate inſtead of being reckoned moiſt would be well entitled to the character of dryneſs. Twenty, or 22 inches is the quantity of rain nearly in Great Britain in a year; but a part of this falling almoſt every day, gives a great degree of moiſture to the air; a great part is abſorbed by the leaves of plants, and moſt of the reſt ſinks into the earth. The climate is, therefore, well entitled to the character of a moiſt one.

As the time in which a given quantity of rain falls, has ſuch different effects, it would ſeem proper in every regiſter of rain to mention particularly, not only the quantity, but how many days, hours, and minutes it continued to rain. If one knew how much rain penetrated the earth in a given time, a rule might be made from which its effects in moiſtening the earth might be diſcovered. The effect within certain limits would be in the compound ratio of the quantity of rain and time it rained. Thus, if ten inches of rain fell in one place in a week, and it rains in another ten weeks in half a year, and the quantity of rain is alſo ten inches, one ſurely could not be very far wrong in ſaying, that the effect of the rain in moiſtening the ſoil and air of theſe two places, would be as 10 to 100, or as 1 to 10. Though in this caſe, we have ſuppoſed the quantity of rain to be exactly equal in the two places, yet ten times

the quantity of rain will penetrate the ſoil where it rains ten weeks, than penetrate it where it rains but one week. Where it rains ten inches in a week, the greateſt part of it will immediately run off the ſoil towards rivers and other ſuperficial ſtreams of water, and ſwell them ſoon to a deſtructive ſize. Very little will ſink into the ſoil, and but little will be evaporated. The air, therefore, will in general be dry. Where it rains ten inches in ten weeks, very little will run off the ſoil, or get immediately into rivers. The greateſt part will ſink into the earth, and either enter into the compoſition of vegetables and minerals, or, after running a great way below the ſurface, will burſt out in the form of ſprings and fountains, whoſe conſtant ſtream will ſupply the rivers gradually with clear water, make them glide gently, ſmoothly, and uniformly along, and neither raiſe them to ſuch a ſize as to become turbulent and deſtructive torrents, nor furniſh a ſupply ſo ſcanty that their channels ſhall become dry.

If one had been diſpoſed to be a little more accurate, ſome allowance ought to have been made on account of the greater degree of dryneſs which the ſoil would acquire in the place where it rained but a week in half a year. We may eaſily ſuppoſe that the ſoil in this dry climate will imbibe moiſture more greedily, and that much more rain would ſink into it in the ſame time. But though this be granted, the effect will evidently be greateſt, when

the rain begins to fall, and will become very little the moment the ſurface of the ground is moiſtened. After every deduction made on this account, it muſt ſtill be admitted, that the effect of the rain in moiſtening the ſoil and air would at leaſt be eight or nine times greater in the places where it rains ten weeks in half a year, than where it rains only one week.

It would therefore be proper to pay ſome attention to the time, as well as to the quantity of rain, if we wiſh to have a proper notion of the moiſture of any country. Though this rule, however, may be uſeful, it is by no means meant that it is perfectly accurate. For there is evidently a certain velocity, with which if rain fall, the whole of it will be abſorbed by the earth. If the rain fall with ſtill leſs velocity in another place, the whole would alſo be abſorbed. If the effect of rain upon the character of a climate were in the compound ratio of the quantity and time it takes to fall, the laſt of theſe places ſhould be much more moiſt than the firſt; becauſe the quantity of rain multiplied by the time gives the greateſt product. In the place where it rains for the longeſt time, indeed, more of the rain will be evaporated, and the air may be moiſter for a while. But independent of this circumſtance, the effect of the rain in moiſtening the earth in both caſes is exactly the ſame, and is proportional to the quantity of rain. If one could diſcover how much rain penetrates the ſoil in a given time, or what

were the differences in this reſpect in different ſoils, it would not be difficult to adapt the rule to all caſes. But it is not meant at preſent to ſuggeſt any more, than that ſomething beſides the quantity of rain in a country muſt be known, before the degree of moiſture in the ſoil and air of that country, or its diſpoſition to rear plants, can be diſcovered. Beſides the quantity of rain and the time it falls, the quantity of dew and ſnow may have influence on the moiſture of a country. The quantity of ſnow combined with the time it takes to melt will have effects ſimilar to violent or gentle rain.

Places where there is ſeldom either rain or ſnow may be ſufficiently moiſt by being ſubject to inundations, from the ſwelling of rivers by rain, which has fallen in another region; or they may be ſupplied with moiſture in a great meaſure by dew. Egypt is a country, in which according to authors, it ſeldom or never rains. It is, however, very fertile, and the fertility depends on the ſoil being partly ſoaked by the annual overflowing of the Nile; the reſt is ſupplied with moiſture from dews, or from the rain which has ſunk into the earth in the higher parts of the country.

The rain which falls in a year is not equally diſtributed throughout the year. In the colder parts of the temperate zone, more rain falls in winter than in ſummer; and in winter, the month of November and end of October, and the month of Ja-

nuary and beginning of February, are wetter than the reſt of the winter months. In almoſt every part of this earth a great deal of rain falls near the vernal and near the autumnal equinoxes. It is not eaſy to account for this unequal diſtribution of rain. Many have endeavoured to explain it, but few have ſucceeded. It may perhaps appear ſurpriſing, that the cauſe of ſo common a thing as rain ſhould have been ſo long undiſcovered, and it might perhaps be conſidered as a proof of the ſmall progreſs which natural philoſophy has hitherto made, were it not that we frequently find in other things that the moſt common are the moſt difficult to explain.

The ancient account of rain is, that the vapour in the clouds was compreſſed, and its particles driven together by the wind, in conſequence of which it immediately fell down in rain. To this Lucretius adds, that the heat of the ſun acting upon the clouds melts rain out of them, and produces it in the ſame manner as wax is melted before the fire. Many of the modern ſolutions are not much more ſatisfactory. It is very generally admitted that the condenſation of vapour, which is raiſed from water by the ſun, is the immediate cauſe of rain. The only objection to this opinion is, that in ſome places more rain falls in a year than is evaporated by the ſun in the ſame time. This, however, does not always happen, and the quantity of rain is as often ſmaller as greater than the water evaporated. The

difference in both caſes is eaſily accounted for. The lightneſs of vapour renders it eaſy for winds to tranſport it far from the place of its formation, and accumulate it in one part and diminiſh it in another.

But though it be admitted that rain ariſes from the condenſation of vapour, ſtill it is not eaſy to underſtand why it falls at one time and not at another. We ſhould rather expect that vapour ſhould be conſtantly condenſing, and therefore that rain ſhould fall every day; for during every day a conſiderable quantity of water is evaporated and carried immediately to a conſiderable height above the ſurface of the earth, where, we know, there is always a great degree of cold. This ſhould condenſe the vapour, and make it conſtantly deſcend in rain. Or if the vapour, as ſeems moſt probable, be diſſolved in air as faſt as it is formed, this air ought, as ſoon as it is cooled, to part with a quantity of that water which the heat had enabled it to diſſolve. We ſhould therefore always have rain when air containing vapour is cooled, and this muſt take place every day. This does not happen, however, ſo far as is known, in any climate. We find indeed that dew falls almoſt every day every where. The condenſation of vapour by cold may therefore, perhaps, be the cauſe of dew, at leaſt of that part of it which falls. But part of what is called dew does not ariſe from condenſed vapours, or from a ſeparation of water from air in conſequence of cold, but is formed from

the condenſation of the watery vapours, which plants perſpire. This vapour is not viſible in the day time, becauſe the air is then heated, and inſtantly diſſolves it. But during the night when the air is generally colder, many plants ſeem to ſmoke, and a part of this vapour condenſes into drops of water. If the night be not colder than the day no condenſation will take place. Let us ſuppoſe that all dew falls in conſequence of the ſeparation of vapour from air by cold: if the night be warmer than the day, no dew ought to fall; if the night is colder at the beginning and warmer towards the end, the dew produced at the beginning of the night by cold may be evaporated again before morning; or the heated air, which kept the vapour diſſolved, and from which dew ought to have been formed by cold, may be carried to a great diſtance by the wind ſhifting and becoming colder and drier.

When wind is very cold, little moiſture will be evaporated; and beſides, in this caſe there is leſs difference between the heat of the day and that of the night; and therefore during cold winds, every thing elſe being equal, there ſhould be leaſt dew. Even during cold winds there may be dew, if the heat of the day and night differ conſiderably. There is generally much dew when the wind blows from the ſouth, eſpecially near the equinoxes, when the difference between the heat of the day and night is greateſt. This is often moſt evident in autumn. In ſummer the heat of the night differs leſs from that

of the day, and the dews are in ſmaller quantity. It may even happen during the continuance of warm winds that there ſhall be no dew, provided the night be as warm, or warmer than the day.

It has been long obſerved, that during north-eaſt, or eaſt winds, there is little dew in this country. Ariſtotle obſerves, the ſame was the caſe in Greece, and that the ſouth wind always produced the greateſt dews. He owns indeed that in Pontus the north-eaſt wind was often accompanied with dew, which he is not ſurpriſed at, becauſe, according to him, the vapour raiſed by the north-eaſt wind is greater than that produced even by the ſouth winds, as is evidently proved by obſerving that wells ſmoke moſt during a north wind.

But though the mere change of temperature be ſufficient to account for dew, it will not explain why rain falls only at certain times. Some have imagined that the principal cauſe of rain ariſes from an effect which the higher parts of the earth have upon air loaded with moiſture. It is a common obſervation that hilly countries have more rain than places where the ground is almoſt level. The tops of mountains have frequently clouds hanging over them, when none are to be ſeen any where elſe. Theſe, it is imagined, are proofs of the high ground attracting the moiſture from the air loaded with vapour, with which it is in contact. But the low or level country, being far below that part of the air to which the vapour is diſpoſed from its

ſpecific gravity to aſcend, has little or no effect upon it. According to this doctrine, rain can only be expected in a low country, when the whole air to the very ſurface of the earth is loaded with vapour. But this account is not quite ſatisfactory. If it were well founded, we ſhould expect that it ſhould rain more or leſs every day on the tops of hills; and when it rained in one part of a level country it might be expected that it ſhould in the whole. But neither of theſe are agreeable to experience. With regard to the firſt, we are certain, that for many days, and even weeks together, there is no rain even on the tops of the higheſt mountains. Neither does this theory help us to explain why in every climate ſome months are wetter than others. Perhaps by a little pains this theory might be ſo far improved, as to agree ſomewhat better with the phenomena; but many difficulties would ſtill remain.

Thus it might be ſuppoſed that wind compreſſes the air loaded with vapour, and diſpoſes it to part with it in the form of rain; that the hills oppoſing the motion of the air make it meet with more compreſſion there than in a low country; that oppoſite winds blowing upon the oppoſite ſides of a hill, and both meeting with reſiſtance, the air on each ſide is greatly compreſſed, and made to part with the whole, or a part of the moiſture which it contained. But though this is Ariſtotle's opinion, and has alſo been revived among the moderns, it does not ex-

plain why rain ſhould fall when there is little or no wind, and why, even in hilly countries, the wind is very ſtrong without rain, and the falling of rain is often found to moderate the violence of the wind. Another cauſe of rain, which has been mentioned, is thunder. It is commonly obſerved that rain or hail follow thunder. It is imagined by ſome, that the ſtroke which the air receives upon the burſting of a thunder cloud brings the particles of vapour in it into cloſer contact; they therefore run inſtantly together by attraction, and rain is thus produced. That ſomething remarkable happens in ſuch circumſtances is proved by obſerving, that when a great number of cannon is fired, as in conſequence of an engagement, the direction or violence of the wind is often altered.

To thoſe acquainted with the effects of electricity more ſpecious reaſons will occur; ſuch as the ſudden contraction of any very light body contained in the air, in conſequence of its electrical ſtate being altered. The ſmoke of roſin and other bodies of a ſimilar kind extends to a great diſtance upon being electrified, either poſitively or negatively, and ſuddenly contracts itſelf upon being reſtored to a natural or quieſcent ſtate. That electricity is one cauſe of rain, receives greater ſupport by obſerving, that rain water ſhews itſelf frequently electrified in a ſmall degree even after it has reached the earth. A late author is diſpoſed from this to explain why

plants watered with rain water, or water which has been long expoſed to the air, grow with greater vigour, and become more perfect in all their parts, than the ſame plants in a ſimilar ſituation watered with well water. He obſerves, however, that this difference ceaſes upon electrifying the well water before uſing it, by ſetting the gardener with his watering pan in his hand upon a cake of roſin, and giving him a communication with an electrical machine in motion. But though theſe circumſtances ſhew that rain, and electricity, or thunder are often joined, yet we frequently find great rains without thunder, and that thunder is generally attended only with violent ſhowers of ſhort duration. Electricity may have ſome connexion with thunder ſhowers, and may be conſidered by many as accounting ſufficiently for them; but it does not ſufficiently explain thoſe frequent and long continued rains, which are not accompanied with thunder, where no extraordinary marks of the electrical fluid in a very active ſtate can be perceived.

Moſt perſons who have endeavoured to account for rain have attempted to uſe only one principle for that purpoſe. I ſhould imagine, that the principal miſtake lies here, and that rain may be produced from ſeveral cauſes, and that all the cauſes which have been mentioned ſometimes may have conſiderable effect in producing it. It ſeldom however happens, that only one of them is concerned

in it. Thunder, and wind, and high mountains, and cold have all at times a tendency to occaſion rain. But all of theſe, excepting the laſt, are rather aſſiſtants than principals, and ſome other circumſtances muſt be taken into account before their full effect can be aſcertained. Of this kind is the alteration in the ſtate of the air before rain falls, and which is indicated by the falling of the quickſilver in the barometer, and the effects ariſing from contrary winds meeting, and mutually affecting the moiſture mixed with the air in each.

The barometer generally falls before rain. The preſſure of the atmoſphere is therefore evidently diminiſhed, and if vapour were merely diffuſed through air, and not diſſolved, it ſhould be diſpoſed to extend itſelf and dilate inſtead of contracting. Notwithſtanding this we commonly find, that rarefaction of the air is accompanied with an immediate ſeparation of the moiſture contained in it. Of this we have a proof from what happens while the air is drawing out from the receiver of an air-pump. At a certain period, during the exhauſtion, the receiver becomes muddy, and drops like dew in a little time ſettle on the ſides of it, and the whole, from being opaque, becomes perfectly tranſparent. If after the air in the receiver has become opaque in conſequence of exhauſtion, the air be ſuddenly let in, the opacity inſtantly diſappears. From this it is evident that rarefying of air containing vapour diſpoſes the moiſture to ſeparate, and as

the falling of the barometer ſhews a ſimilar rarefaction in the atmoſphere, the ſame ſeparation of water ſhould take place. The reaſon why the diminution of preſſure in air containing vapour diſpoſes it to part with its moiſture, and produce rain, I ſhall now endeavour to explain.

Soon after the barometer was invented, it was obſerved that the mercury in it did not always ſtand at the ſame height. Theſe alterations in height were ſoon imagined to be connected with variations in th weather, in conſequence of which barometer and weather-glaſs became ſynonymous terms. The range of the mercury in Europe at the level of the ſea is about three inches; at each half inch of which is placed a word expreſſive of the weather it indicates. After many obſervations, however, it has been found that the ſame kind of weather does not invariably occur, when the mercury ſtands at the ſame part of the ſcale. But notwithſtanding many occaſional irregularities there is in general reaſon to expect fair weather, when the mercury riſes, and its fall is frequently, though not always, followed by rain. Why the ſinking of the mercury is commonly ſucceeded by rain is a queſtion which has intereſted the natural philoſophers. They ſeem in general to have admitted the method of accounting for this phenomenon, which was firſt propoſed to them, without examining very minutely into it. This explanation appears to me to be attended with ſome difficulties. The principle on which this

theory depends, is that a great quantity of vapour is conſtantly riſing from all the parts of this earth; that this vapour is mixed with the air, and that this mixture of air and vapour is heavier than pure air. As long as the barometer remains at the height at which it ſtood when the vapour was formed, it is ſuſpended. When the barometer falls, and the air becomes lighter, the vapour can no longer be ſuſpended, but deſcends in the form of rain or ſnow, according to the particular heat of the weather at the time. One great difficulty which attends this hypotheſis is, that it is not eaſy to conceive upon theſe principles how the vapours can aſcend to ſo great a height in the atmoſphere. The air at the ſurface of the earth, it is aſſerted, becomes heavier than it was before, in conſequence of being mixed with vapour. One would expect this mixture of air and vapour to hover on the ſurface of the earth, inſtead of riſing to ſuch a height above it, as experience ſhews that it does. This is perfectly contrary to what happens in other caſes, where we always find that the denſer body ſinks inſtead of riſing through the rarer. In the preſent inſtance the vapours are in greater quantity at a conſiderable height above the ſurface than at the ſurface of the earth.

In order that the vapour of water may aſcend into the ſuperior regions of the atmoſphere, it is neceſſary to admit, that vapour, and even air mixed with vapour is lighter than pure air; and that it is ſo ſeems

evident from other circumſtances beſides their riſing to ſo great a height. The vapours produced from water at the boiling point, have been found by experiment to occupy eighteen hundred times the ſpace which the water they were produced from occupied. If the denſity of the vapours of water produced in the ordinary heat of the weather be the ſame, theſe vapours ſhould riſe to a great height in the atmoſphere. For the denſity of the air at the ſurface of the earth in the mean temperature of the weather is to that of water as 1 : 850, and the denſity of vapour at the boiling point is to that of water as 1 : 1800; whence the denſity of air is to that of vapour, as 1800 to 850. Theſe vapours would therefore aſcend, independent of reſiſtance and rarefaction, from diminution of preſſure, till the air became of the ſame denſity with them, which it would be at that height above the ſurface of the earth, when the air could occupy more than double the ſpace which it does at preſent, or where the mercury in the barometer would ſtand at about fourteen inches. Suppoſing it to ſtand at 30 inches at the level of the ſea, this would be at the perpendicular height of four miles above the ſea, or higher than the higheſt mountains in this globe. That vapours really riſe to a great height, is evident from the tops of the higheſt hills being perpetually covered with ſnow. The whole of the vapours raiſed from water do not however riſe ſo high, on account of their progreſs being ſtopped by the reſiſtance which they meet with

in their paſſage, and their being prevented from riſing perpendicularly by the force of the winds.

The air then in conſequence of being mixed with vapour ought to be lighter than pure air, and experience has proved it to be ſo. If ſpontaneous vapour be not ſo rare as that produced at the boiling point, and if the effect of heat upon it be the ſame as on air, the vapour formed in the ordinary heat of our weather ought to be in denſity to the air as 850 : 1350. The denſity of the air is the ſame with this, when the barometer ſtands at twenty inches, at a height about two miles above the ſurface of the earth. But vapour riſes higher than this, as is evident from the obſervations of the French academicians in the neighbourhood of Quito.

All experiments confirm the opinion, that vapour is much lighter than air. So much ſo that if it preſerved its form, in the firſt caſe the barometer muſt fall to fourteen inches before it could reach the ſurface, and in the ſecond to twenty inches before this could happen. But as the barometer ſeldom falls lower at the level of the ſea than 28 inches, theſe vapours, if no change happened to them, would never fall down again to the earth. We find, however, that they do fall down, and how this ſhould happen is more difficult to conceive.

One cauſe of the deſcent of vapour in the form of rain, in conſequence of the barometer falling probably depends on the nature of vapour with regard to heat. It is well known that vapour contains much

more heat in it than the water from which it was produced, or, what is the ſame thing, that it requires more heat to heat vapour to any degree of the thermometer than to heat water to the ſame degree. And it has lately been found by experiment, that the difference is greateſt when the difference of denſity is greateſt: that is, there is more heat neceſſary to preſerve vapour in the form of an elaſtic fluid, when it occupies a large ſpace than when it occupies a ſmall ſpace. But the ſpace which vapour occupies, as in all elaſtic fluids, is reciprocally as the force which compreſſes it. It will therefore require more heat to keep water in the form of vapour, when the barometer is low, than when it is high. That this is the caſe is evident from many experiments. If a quantity of vapour is included in the receiver of an air-pump, and a thermometer along with it, and a part of this vapour be drawn out by working the pump, the thermometer will ſink as the quantity of vapour diminiſhes, and the remainder expand ſo as ſtill to fill the receiver. After a certain time, however, the thermometer ſuddenly riſes to the ſame degree at which it ſtood at firſt. But then the vapour divides itſelf into two parts, one becomes water immediately, and in doing ſo gives out a ſufficient quantity of heat to heat the vapour to the temperature which it had at firſt. A part of this experiment every perſon has ſeen in the common exhauſtion of an air-pump. The air in the receiver is at firſt perfectly tranſparent; when the pump is

wrought a little, it becomes muddy, and a number of drops of water condenſes on the ſides of the receiver.

At any given height of the barometer, there is a great quantity of vapour mixed with the air, and this mixture is lighter than pure air. This vapour is of the ſame temperature with the air in its neighbourhood. The moment the barometer falls, this vapour dilates itſelf. It muſt immediately either become much colder than before, or if it remains of the ſame temperature, it muſt divide itſelf into two parts. One part deſcends in rain, and the heat which this gives out ſerves to keep the remaining vapour of the ſame temperature which it had before the dilatation took place. We find accordingly, that this diviſion of the vapour into two parts takes place principally in the higher parts of the atmoſphere, and it is well known that more rain falls on the tops of hills than on an equal ſurface of ground lower down. Why this diviſion of the vapour does not always follow the falling of the barometer is not altogether ſo eaſily explained, though ſomething of this kind might be attempted with ſufficient plauſibility.

The laſt ſource of rain, which I ſhall mention, is the effect which two ſtreams of air differing in temperature have upon the moiſture, which each ſeparately contained. This effect has been frequently obſerved. Every perſon muſt have noticed, that rain generally ſucceeds a ſudden change of the wind, and that it is very ſeldom that rain falls in great quantity,

unleſs when this happens. The effect of this circumſtance too is moſt remarkable, when the wind changes from a quarter which is accompanied by cold weather, to one that generally brings warm weather. The ſhifting of the wind to the ſouth or ſouth-eaſt, after it has blown from the north, is almoſt always followed by rain. A ſudden change from ſouth to north may alſo produce the ſame effect. But though theſe obſervations have been made by almoſt every perſon, the reaſon why rain is thus produced has been underſtood by few. The direction of the wind is often very different at the ſurface of the earth, and at a ſmall height above it. We frequently ſee the clouds moving in direct oppoſition to the current of wind below. The clouds often appear to come from the north-eaſt, while the wind blows from the ſouth-weſt; and we may ſometimes obſerve, that the direction of the upper and under current of air ſuddenly change places. The wind being ſouth-weſt, and the clouds moving from the north-eaſt, ſuddenly ſhift, the wind blowing from the north-eaſt, and the clouds coming from the ſouth-weſt. The air ſomewhere between theſe two currents muſt be in a great meaſure ſtationary. Theſe oppoſite currents of air, when they ſhift in the manner which has been juſt mentioned, are almoſt always attended with rain: but the rain is ſeldom conſtant, falling in heavy ſhowers, with intervals of fair weather.

It is not improbable, that theſe oppoſite currents of

air frequently take place when we do not perceive them, and the adventures which the late aëronauts have met with put this matter almoſt beyond a doubt. A balloon, after riſing to a ſmall height, often moves in a very different direction from what it did when near the ſurface of the earth, and this has happened when no difference in the direction of the under current could be otherwiſe diſcovered. The conſequence of ſuch oppoſite currents of air, each containing a quantity of moiſture, will be more or leſs ſudden, according as they move towards each other with more or leſs velocity, or differ more or leſs in their heat. If the adverſe current, when uppermoſt, move very ſlowly, ſcarce any other change will commonly be produced than the ſeparation of a little moiſture from the air, and afterwards a greater degree of tranſparency in it than before. The country people obſerve, that before rain diſtant hills appear more diſtinct, and apparently are nearer than they were before; and Mr. Herſchell has remarked, that in uſing teleſcopes with a great magnifying power, he does not ſee diſtinctly till the air has begun to part with its moiſture, and the tube of the teleſcope becomes wet with dew.

If the two currents of air differ very much in temperature, the change of weather will be more ſudden with the ſame motion in the two currents towards each other. In the early froſts towards the end of Autumn, the change from froſt to rain is very frequent and ſudden. The country people, who are

all experimental philoſophers, have obſerved this. When this happens, the wind, from being northerly, becomes ſuddenly ſouthernly, and the different direction of the currents is moſt remarkable. Before the ſetting in of the froſt, about Chriſtmas, and for ſome time after it, in the month of January, the difference of the temperature of a north and that of a ſouth wind are perhaps at a maximum; and great quantities of rain or ſnow uſually fall in the month of November, and at the end of January or beginning of February. This effect of two currents of air differing much in heat, is moſt evident at the end of a froſt, and is uſually accompanied with a great deal of rain. But this is not abſolutely neceſſary. It may happen, that a thaw may be produced by a change of wind towards the ſouth, without there being any current of colder air in an oppoſite direction. We need not be ſurpriſed, that ſuch a thaw ſhould be accompanied with little or no rain.

Theſe obſervations, which every perſon muſt have made, ſeem to ſhew, that two currents of air meeting, have a conſiderable ſhare in the production of rain. Dr. Hutton, of Edinburgh, has attempted to ſhew, that air heated, when ſuddenly mixed with a quantity of colder air, immediately will be attended with a ſeparation of water. He has not confirmed it hitherto by experiment; but it ſeems not improbable, that the quantity of vapour, which a gallon of cold and a gallon of hot air can keep ſuſpended, when ſeparate, is much greater than two gallons of

air of the mean temperature can retain, and that in conſequence of the mixture a quantity of vapour will ſeparate. This we find to take place in a ſimilar inſtance in water and nitre. A pound of cold water and a pound of hot water, each ſaturated with this ſalt, have been found by experiment to keep more ſalt diſſolved when ſeparate than two pounds of water of the mean temperature would do. Thus a pound of water at 32° will diſſolve two ounces and a half of nitre: a pound of boiling water will diſſolve twenty-four ounces. Two pounds of water thus keep in ſolution twenty-ſix and a half ounces of nitre. If theſe two ſolutions are mixed, a quantity of the ſalt ſeparate; becauſe two pounds of water heated to 212° will not diſſolve twenty-ſix ounces and a half of nitre, but only ten ounces. Reaſoning from analogy, which however is not much to be truſted in phyſical ſubjects, we may judge it not improbable, that ſomething of a ſimilar nature may take place in mixing cold air ſaturated with vapour, with hot air ſaturated with vapour, and that this may help us to explain the phenomena already mentioned. We cannot however accept of it as a complete ſolution, till it has been fully eſtabliſhed by experiment.

But though the effect of contrary winds in diſpoſing air to part with moiſture were greater than is pretended, this cannot with any probability be uſed to account for rain in every ſituation. It may in part explain why a greater quantity of rain ſhould fall in winter than in ſummer, and near the equinoxes

than before or after them. But it does not explain the rain, which falls in the middle of the torrid zone, where winds of different temperatures are not ſo common. Neither is the effect of hills in producing rain entirely accounted for by it. But this, though perhaps the great cauſe of rain, is often counteracted by other cauſes, ſuch as the ſtate of the air with regard to denſity and electricity, each of which is ſufficient alone to produce rain, or to prevent it even when contrary winds favour it. The effect of hills in producing rain ſeems to depend in a great meaſure on the air about them being very cold, and this is the caſe even in the torrid zone. When the wind is warm, and contains much moiſture, as it comes in contact with a hill, it mixes with air much colder than itſelf, and immediately the moiſture it contains is ſeparated. If this be held a ſolution of the greater quantity of rain in hilly countries, there ſhould be leaſt rain in them, when the wind blows from a cold quarter, ſuch as the north-eaſt, for a long time.

NOTE BY THE EDITOR.

It is to be obſerved in this eſſay, that the quantities of rain ſtated to have fallen in various places are not always preciſely the ſame as they are given in other authors. I have no doubt that the number of inches was taken upon ſome good grounds, probably upon the authority of experiments. I do not however know what theſe were. It is of the leſs importance to aſcertain theſe points

ſince the ſpeculations contained in this eſſay do not at all reſt upon theſe numbers, which are merely occaſionally quoted for the ſake of illuſtration, and not as the ground-work of any important argument. There has been perhaps for the laſt twenty years leſs improvement in the theory of rain than in moſt parts of natural ſcience. The opinions here announced will appear proportionally more like the preſent doctrines, and the addition offered to the cauſes of rain will be more likely to approach to the truth. It ſeems indeed to me ſcarcely poſſible to deny to the rarefaction of the air of the atmoſphere, the power of producing a depoſition of moiſture, nearly in the manner above illuſtrated. Whatever opinion may be entertained of the method in which that is effected, the experiment of dilating air in the air-pump, puts it beyond the reach of controverſy, that a fall of the barometer muſt always be accompanied with rain, unleſs more powerful counteracting cauſes interfere to prevent ſuch precipitation.

ESSAY V

ON THE FERTILITY OF SOILS.

Read A. D. 1778.

THE ſoil in which plants grow, conſiſts principally of earth mixed with a ſmall quantity of the remains of rotting vegetables. Earth forms by far the greateſt part of every ſoil. The quantity of vegetable matter is different in different ſoils, and even in the ſame ſoil it varies at different times. Beſides earth and vegetable matter, every ſoil contains a ſmall quantity of ſalt, which, though ſometimes it be purely ſaline, is very frequently of an earthy or metallic nature. The quantity of ſalts of every kind mixed with the ſoil, is always very ſmall. If it were conſiderable, the fertility of the ſoil would be entirely deſtroyed. Every ſoil principally conſiſts of a confuſed mixture of thoſe earths which lie in regular ſtrata immediately below it. It differs, however, much from theſe earths, not only by their

wanting that vegetable matter which the ſoil contains, but by being in a different ſtate before and after their expoſure to the air. When earth is thrown up from a conſiderable depth below the ſurface, it is long before any vegetable will grow in it. It gradually ſwells, and crumbles down, and after a certain time, is much more ſoft and ſpongy than it was before. This is moſt remarkable in the ſofter earths, but it may be obſerved even in the hardeſt. The change on the latter, however, is more ſlow and gradual. Even the ſcoriæ of volcanos, though little ſhort of being in a ſtate of vitrification, fall in time in the air, ſo as to allow thoſe plants that require but little ſoil to grow on them. The remains of thoſe plants mixed with what had formerly mouldered from the lava, and what has ſince mouldered, form a ſoil of a greater depth, fit for the nouriſhment of vegetables of a different claſs, which require a deeper ſoil, and thus by ſlow degrees, a ſufficient thickneſs is obtained for the growth of the moſt perfect plants.

This has repeatedly taken place, as is evident from obſervations on the ſoil in the neighbourhood of Naples. The ſame thing that happens to the lava of volcanos, would happen to all the rocky ground in the country, were it not that on account of the rocks being for the moſt part greatly inclined to the horizon, the greater part of that which moulders in conſequence of expoſure to air, is waſhed down by the rain, or blown away by the wind. Thus the ſoil

does not accumulate, and is only fit for the ſupport of lichens and moſſes.

The ſoil derives its character principally from the ſtratum of earth immediately below it; though it is not entirely compoſed of it. Sand and gravel, the calcareous earths and clays, with ſometimes magneſia, form the principal, if not the whole of the earthy part of every ſoil. In ſome caſes, theſe are mixed with much animal and vegetable matter, and frequently the ſoil conſiſts of the roots, ſtalks, and leaves of plants mixed with a quantity of water. Hence ſoils have been divided into the ſandy or gravelly ſoil, into the chalky ſoil, into clayey ſoils, and into the loamy, by which laſt is meant a quantity of animal and vegetable matter mixed with ſand or clay. To theſe may be added the moſſy ſoil, conſiſting principally of the roots, ſtalks, and leaves of plants in a ſtate of imperfect preſervation mixed with water.

To each of theſe diviſions a number of ſoils differing in ſome reſpects from one another belongs, though the article from which they receive their claſſical name be the prevailing ingredient in the compoſition of all. Theſe ſoils differ in the nature of the plants belonging to each one, which grow with greater vigour, and arrive to greater perfection in it; and which degenerate or die if removed into a different one. It is in dry gravelly ſoils that fir and other reſinous trees arrive at their greateſt perfection, and grow moſt vigorouſly. They are unhealthy and do not attain ſuch ſize in a clayey ſoil,

of plants the ſoil may contain, but little of it can be tranſmitted to the vegetable on account of the ſmall ſurface which the roots of the plant expoſe to the ſoil from which the nouriſhment is drawn. In theſe reſpects, different ſoils differ extremely from one another. In ſandy and gravelly ſoils, as they conſiſt of parts entirely incapable of being affected by water, the moiſture is readily imbibed, and as readily paſſes through them, or they part with water as readily as they receive it; the roots of plants inſinuating themſelves with eaſe between the particles of which the ſoil is compoſed.

A clayey ſoil conſiſts of parts eaſily affected by water, in conſequence of which, upon being moiſtened, they ſwell to a conſiderable ſize. Water, however, does not penetrate them as readily as it does ſand or gravel. A quantity of water thrown on dry clay, moiſtens only its ſurface. The greateſt part runs off, while the internal parts remain nearly in the ſame ſtate as before. When clay is thoroughly wet, it parts with its water very ſlowly; it contracts in proportion, becomes harder and firmer, while many cracks form themſelves in it. The roots of plants penetrate it with difficulty. The effects of water on theſe ſoils are oppoſite. Hence a quantity of clay thrown on a ſandy ſoil, by hindering the water from paſſing ſo readily through it, enables it to nouriſh plants more perfectly than it would have done without this mixture. And, in the ſame manner, when a quantity of ſand is added to a clay

ſoil, its parts are divided; its tenacity is diminiſhed; the roots of plants penetrate it with great eaſe, and it both receives and parts with water more readily than before.

Moſt ſoils conſiſt principally of theſe earths, either ſeparate or combined, and thoſe ſoils are naturally moſt fertile which conſiſt of a mixture of both, and where there is a little more ſand than clay. By breaking the ſoil into ſmall parts by mechanical means, the interſtices of the particles become ſmaller and more numerous, and better calculated to retain and tranſmit moiſture, while the roots of the vegetable penetrate the ſoil with greater eaſe, and ſpread themſelves by this means to a greater diſtance ſo as to draw more nouriſhment. Ploughing or digging the earth has univerſally been allowed to promote its fertility, and that every advantage in vegetation depends on the proper tranſmiſſion of water is evident from the experiments of Mr. Bonnet, of Geneva, who reared many plants by putting the ſeeds in cotton, or ſponge, or moſs, and watering them from time to time. The plants would not grow to equal perfection in water, becauſe they would by this means have abſorbed more water than is neceſſary for their moſt perfect growth, while the water, when the roots of plants are immerſed in it, is apt to ſpoil them. Every vegetable, from Hale's experiments, requires a certain quantity of water for ſupporting it in the moſt perfect ſtate. If more or leſs than this is allowed, the vegetable decays, and

equally ſo by too great a quantity as by too little. In Mr. Bonnet's experiments, the vegetables did not merely ſend forth ſtalks and leaves, but continued to grow with vigour, ſo as to produce fruits and ſeeds in perfection, the great points to which all vegetation tends, and on account of which ſolely, the ſtalk, leaves, and flowers are produced. He raiſed vines in this manner, which produced grapes not inferior in flavour to thoſe produced in the richeſt ſoils in the neighbourhood, and the lemons and oranges growing in ſponge moiſtened with water, were equal in every reſpect to thoſe produced in the beſt ſoil, and with equal attention. Great care no doubt, was taken in diſtributing water to the vegetable in theſe experiments. Without this, it is impoſſible he could have raiſed ſuch perfect plants. He probably imitated nature in this reſpect. He would allow them plenty of water at the beginning; this quantity he would gradually diminiſh, as Hale's experiments ſhew that the abſorption of moiſture by plants, diminiſhes in proportion as the fruit approaches to maturity, and ceaſes almoſt entirely when that is completed.

Vegetables abſorb likewiſe a conſiderable quantity of air. This they do partly by their roots, and partly by their leaves. The air abſorbed, is either common air, or air of a different nature, ſuch as has contributed to the inflammation of combuſtible bodies and reſpiration of animals, or which is ſeparated from animal and vegetable bodies during their pu-

trefaction. The leaves of plants ſeem to abſorb common air in conſiderable quantity, but they abſorb likewiſe the other kind, or fixed air, or rather ſeparate from it that part which it contains, upon which its difference from common air depends. The ſoil in which vegetables grow contains a quantity of atmoſpheric air between its particles, but this probably is not ſolely that which the roots of plants abſorb; for, if this were the caſe, every ſoil which tranſmits water equally, ſhould be equally fertile; but this is not agreeable to experiment. Pure earth, and earth taken up from a conſiderable depth, that has never nouriſhed a vegetable, is not fertile. Few plants will grow in it, and thoſe of the moſt imperfect kind. Yet, there is as much common air between the particles of this ſoil, as in ſoils that are more fertile. By expoſing theſe ſoils to the air, they in time, acquire a vegetative power, partly by attracting air, and partly by catching that vegetable and animal matter which is floating in the atmoſphere.

From every ſoil which is fertile, a quantity of air of another kind, the fixed air, can be ſeparated, which is produced by the vegetable and animal matter contained in it, and is produced by the gradual putrefaction of that matter. It is from wanting this matter, that pure earth is unfit for the growth of plants, and, that ſoil brought from a great depth, is ſo long in becoming fertile. This vegetable matter

contained in every ſoil, affects its fertility in two ways, partly by modifying the power of tranſmitting water to the vegetable, and partly by furniſhing air to it. It diſpoſes ſand to retain more moiſture for a longer time in it, and to part with it more gradually. By dividing the particles of clay, it renders it more diſpoſed to receive and part with its moiſture, and more eaſily penetrated by the roots of plants; when left to itſelf, it gradually rots, and furniſhes a quantity of air ſufficient to ſupport a certain number of plants. If, by any means which haſten its putrefaction, a quantity of air is produced from it, a greater number of plants will grow in it; a greater crop will be produced, but in time, the whole vegetable matter being deſtroyed, the fertility ceaſes, and the ſoil is reduced to the ſtate it was in before any plant had grown in it. We can reſtore its fertility, by mixing it with a quantity of putrid animal or vegetable matter.

Vegetables differ greatly from one another in the quantity of vegetable matter which they leave in the ſoil, after the part above ground has been ſeparated. Some of them leave more vegetable matter than was employed in furniſhing them with air: but this in time produces a change in the ſoil, with regard to its power of tranſmitting water, which renders it unfit for the perfect ſupport of the vegetable, in conſequence of which, the ſoil ceaſes to be fertile for that vegetable. Others leave leſs vegetable mat-

ter than that employed in furniſhing them with air, on account of which, the ſame ſoil ceaſes in a ſhort time to be fertile. By raiſing theſe plants alternately in the ſame ſoil, their effects, in a great meaſure, correct one another, and a greater crop can, for a number of years be thus obtained, than by raiſing only one of them. It is principally in this way that the fertility of the ſoil can be preſerved for a long time, by raiſing the ſiliquoſe or leguminous plants, alternately with the annual culmiferous plants. Some plants, as trees, produce little alteration in the quantity of air contained in the ſoil, and can, therefore, grow in the ſame ſoil a long time. Theſe require little air for their ſupport, at leaſt, abſorb little by their roots, and are, therefore, raiſed moſt eaſily by water, as is evident from Van Helmont's and Bonnet's experiments, where little air could be attracted by the roots, the leaves attracting the greateſt part neceſſary for vegetation.

Vegetables may be injured by a too great quantity of air tranſmitted to them, as well as by too much water. The uſe of putrid animal or vegetable matter mixed with the ſoil is therefore limited. When uſed, however, in a moderate quantity, they afford the moſt certain method, at all times, of promoting the fertility of the ſoil.

Frequently, the vegetable matter contained in the ſoil has ſome ſmall remains of vegetable life. While it retains this, it does not rot, or furniſh air. By

diſturbing the ground frequently, ſo as to hinder any vegetable from growing in it, we deſtroy theſe remains of life. Many ſubſtances, known by the name of manures, owe their efficacy to their power of extinguiſhing this vegetable life, and diſpoſing the matter to rot, and produce air. The principal ſubſtances of this claſs are calcareous earths or marls, the aſhes of burnt plants, and common ſalt. The firſt of theſe genera contains chalk, ſhells, and lime, or lime-ſtone. Calcareous earth may increaſe the fertility of the ſoil in three ways. Firſt, by altering the ſoil with regard to its power of tranſmitting water: ſecondly, by decompoſing ſalts which the ſoil contains, and which are noxious to vegetation; and laſtly, by promoting the production of air, from the vegetable and animal matter in the ſoil. The firſt of theſe it will produce moſt remarkably in ſandy ſoils, by rendering them more compact, and more diſpoſed to retain water. It produces its effect, in a certain degree, upon clay, by dividing its particles. It is ſeldom, however, that it is mixed in ſuch a quantity with the ſoil, as ſenſibly to produce either.

The ſecond effect it does not produce at all in ſandy ſoils, for they ſeldom contain any thing ſaline. But it produces it remarkably in clays, as many common clays contain a conſiderable proportion of vitriolic ſalt of iron. This is very noxious to plants; they are always ſickly, and of a ruſty hue when they

grow in ſoils containing this. The calcareous earth decompoſes this ſalt, and ſeparates the acid, converting it into a ſalt leſs noxious to vegetables, and leſs ſoluble in water.

The third effect the calcareous earths produce in a very remarkable degree, as is evident from Pringle's experiments, where vegetable and animal ſubſtances, mixed with calcareous earths, always rotted ſooner, and furniſhed a greater quantity of air in the ſame time, than they did, when expoſed, in ſimilar circumſtances, without that addition. Calcareous earths are uſed as manures, either in the form of lime, or in their crude ſtate. When applied to the ſoil in the form of lime, they ſoon attract from the air that ſubſtance which they had loſt by the burning, and operate, therefore, on the ſoil, in the ſame manner as if they had been in their crude ſtate. The only uſe of burning the calcareous earth into a lime for manure is, to reduce it into a fine powder with greater eaſe than could have been effected by mechanical means. Common ſalt, though it be generally employed to preſerve animal ſubſtances from corruption, has been found to promote putrefactions, when uſed in ſmall quantities. The effects which it produces on the ſoil are the ſame with the effect of the calcareous earth. The more vegetable and animal matter is contained in any ſoil, the greater effect will be produced on it by the calcareous earth and common ſalt. If the ſoil contain no vegetable or animal matter, they produce no ſenſible effect.

If they promote the corruption of the vegetable and animal matter in the ſoil, they leave leſs of them in it, in a given time, than it would have contained, if the ſoil had been left to itſelf. They promote the fertility of one year, by diminiſhing the materials neceſſary for that of the next, and, if uſed too frequently, will, in time, deſtroy the fertility altogether, as has happened on many occaſions. When the ſoil is thus rendered barren, it cannot be made fertile in any other way than by mixing with the ſoil a quantity of vegetable and animal matter, unleſs by leaving it to itſelf, when it will, after a long time, ſupply itſelf with vegetable matter, like the ſoil which has been brought from a great depth, in which no plant had ever grown.

NOTE BY THE EDITOR.

It is now believed by many, that plants abſorb by their roots ſome liquid vegetable matter held in ſolution by water, in a manner not very clearly underſtood, but of which the exiſtence is very probable. It is indeed obvious to any perſon, who looks at the water draining from a heap of manure, that carbonaceous matter is held in ſolution. There have been also experiments not wholly agreeing in result with Van Helmont's and Bonnet's, by Haſſenfratz and others, from which it has been concluded, that ſeeds and bulbs furniſh the carbon of which plants are formed when they grow in water, and that in Van Helmont's

celebrated experiment, carbonaceous matter had access through the sides of the earthen pot. By these means some doubt has been thrown on the origin of the hard substance of plants, but the subject is by no means perfectly elucidated. The experiments of Bonnet are too striking to be given up without further investigation.

An allusion is made in this essay to the power of plants to decompose fixed air, and to restore it to the atmosphere in its original state. It is here proper to observe, that Dr. Irvine always held himself to have been the first who suggested the probability of this power of vegetation to renew the purity of common air, by reducing carbonic acid to its principle.

ESSAY VI.

ON THE SEEDS OF PLANTS.

PLANTS are compoſed of a great number of veſſels adjuſted to each other with the greateſt exactneſs. By theſe they are fitted for receiving and tranſmitting different fluids. They are furniſhed with organs by which the nouriſhment which they receive from earth, from water, air, and light, is changed into new compounds, ſo as to form juices peculiar to each plant. No art is capable of ſeparating from the ſubſtances by which vegetables are nouriſhed, products in any reſpect ſimilar to thoſe prepared in the bodies of vegetables themſelves. Different vegetables nouriſhed by the ſame ſoil, water, air, and light ſtill preſerve their ſpecific differences, not only in their external form, but in their intrinſic qualities.

Many attempts have been made to diſcover in what manner vegetables were capable of ſeparating from the ſubſtances by which they are nouriſhed, juices peculiar to them. The opinion which has been moſt commonly entertained on this ſubject is, that the ſoil contained all the parts found in vegetables, and that each plant had a power of ſeparating from it that matter which beſt ſuited its particular nature. This notion has not, however, been ſupported by experiment; nothing has been found in the ſoil excepting water and air. The various juices found in different plants are evidently derived from another ſource, as I have formerly endeavoured to ſhew.

After the ſtructure of the human body had been for ſome time carefully inveſtigated, it was found that many of its functions could be explained immediately from its ſtructure. The operations in vegetation, it was imagined, might likewiſe be better underſtood by a ſimilar inquiry into the ſtructure of plants. Many perſons accordingly examined their ſtructure and many curious diſcoveries have been made in the vegetable œconomy. Much, however, ſtill remains to be done on this ſubject, before the various operations in vegetation can be fully underſtood. The diſcoveries which have been made concerning the ſtructure of plants are very numerous; it is impoſſible to mention them all in one eſſay: A few obſervations on the ſeeds of plants will form my preſent ſubject.

The propagation of the ſpecies is the great end of all vegetation. Every part of which vegetables are compoſed tends directly to this point. It is to effect this, or to be conducive towards it, that plants have roots, ſtalks, leaves, and flowers. Of theſe the flower is the part moſt immediately connected with the propagation of the ſpecies. The formation of the ſeed depends entirely on it. Whenever flowers appear on plants, we are certain that the ſeed is immediately to be formed; but the roots and leaves of plants are not ſo immediately connected with the formation of the ſeed, though they are likewiſe uſeful in this reſpect. A vegetable may produce ſtalks and leaves for a long time without producing any ſeed.

There are great differences among vegetables with regard to the time they muſt grow before the flower and ſeed make their appearance. In ſome plants the ſeed is produced always in the firſt year of the growth; in ſome the flower does not appear till the ſecond year; and in others the plant muſt produce leaves for a great number of years, before it acquires ſufficient ſtrength to ripen its flower, and ſend forth its ſeed. When plants have produced their ſeeds in the greateſt perfection, the powers of vegetation immediately become languid, and are no longer exerted with their wonted vigour. In ſome plants the moment the ſeed is completely formed, the vegetation entirely ceaſes, and never can again be renewed, but the plant totally withers and dies. In

others, after the ſeed is formed, the plant languiſhes for ſome time, and then after reſting a while, recovers its power of vegetation, ſo as to produce ſeed a ſecond or a third time, and in ſome for many hundred times.

The period during which the vegetation goes on in this languid manner in thoſe vegetables in which it is capable of being renewed, is different in different plants; it even differs in the ſame plant according to the warmth of the climate in which it grows. In ſome caſes, even in the ſame plant, one part of it may be covered with flowers and growing with vigour, while another part of it is in that languid ſtate, which conſtantly follows the ripening of the ſeed. This takes place chiefly in the ſtronger plants, which have a woody ſtem, and grow to a great ſize. The trees which grow in the torrid zone have frequently flowers on one part, while the reſt is loaded with ripe fruit. Sometimes trees are found in this ſituation even in this climate, but it never happens excepting in uncommonly warm ſeaſons.

In thoſe plants which grow vigorouſly, and produce ſeed and languiſh alternately, the flower and ſeed, which appear in any one ſeaſon, are not the produce of that ſeaſon. The flowers which are to appear the next ſeaſon are all completely formed already; they begin to be formed in Auguſt, and are at the cloſe of the year, when the ſeaſon is ſo cold that all vegetation ſeems to be at an end, increaſing in ſize, and gradually unfolding their parts.

Even the leaves of plants, which are to make their appearance with the returning ſpring, are then perfectly formed, and require only the aſſiſtance of a little warmth to make them expand, and this happens not only in the ſtronger and more rigid plants, ſuch as trees and ſhrubs, which endure the cold of winter without injury, but to plants of a more tender kind, which die to the ground in winter. In theſe plants the leaves and flowers of the ſucceeding year are formed at the end of the ſeaſon on their roots, which being covered with a quantity of earth are thereby ſecured from the winter's cold.

Thoſe plants which produce flowers and ſeeds but once during the period of their growth, are ſolely employed in producing leaves and flowers, and ripening their ſeeds. Whereas the plants which produce flowers and ſeeds a great number of times with intervals, during which their vigour diminiſhes, have a much more complicated taſk to perform. In any one year they have flowers to expand and ſeeds to ripen, which they had formed the laſt year, and the flowers to form which they are to produce with ſeed to be ripened the next year. As they have more to perform, they require more ſtrength to be able to do it; and it is not improbable that it is owing to this that moſt plants which are capable of producing ſeeds a great number of times, are ſeveral years after they are raiſed from the ſeed incapable of producing either flowers or ſeeds, but increaſe in ſize or ſtrength, after which they begin to propagate the ſpecies. In

this reſpect theſe plants reſemble in ſome degree the more perfect animals, in which it is ſome time after their formation before they arrive at the age of puberty, ſo as to be capable of propagating their ſpecies. Whereas the plants, which can produce ſeed but once, and which do it the year that they are raiſed from the ſeed, have ſome reſemblance to the more imperfect animals. The plants of this claſs are commonly called annuals; thoſe of the other are called perennial plants.

There is perhaps ſome difference in the ſtructure of the ſeeds of the plants in theſe two claſſes. It ſeems not to be improbable that the ſeed of an annual plant is more perfect than the ſeed of a perennial one. The whole of vegetation in the annual plant is directed to the formation of the ſeed. In the perennial one, the vegetation is directed partly to this point and partly to the formation of the leaves and flowers of the ſucceeding year.

As the perennial plant has more to perform, it may, notwithſtanding its greater ſtrength, form the ſeed, which is but a part, leſs perfectly than the annual plant can form its ſeed, which is the whole intention of its growth. That this is really the caſe, ſeems to be evident from the reſemblance there is between the buds of perennial plants and the ſeeds of annual ones with regard to vegetation. The flowers and ſeeds of perennial plants which are produced for the ſervice of the next year, are moſt curiouſly fitted up, and appear in the form of little

conical ſubſtances called buds or germs. If we take the buds and put them in ground, they frequently take root, grow, and produce a plant exactly reſembling, in appearance, the parent plant; but which differs from it in one reſpect, that it produces and ripens its ſeed much ſooner than a plant of the ſame kind raiſed from the ſeed. Very frequently a plant raiſed in this manner will produce ſeed, and propagate the ſpecies the firſt year of its growth, while the ſame plant raiſed from the ſeed would have required five or ſix year's growth before it was capable of propagating the ſpecies. That this is the caſe, the conſtant practice of all gardeners is a proof, who know well that when they want to furniſh a garden with perennial plants, there is not a more certain method than to raiſe the plants from ſlips. This practice ſucceeds beſt in thoſe plants whoſe trunk dies down to the ground in winter, and where the buds adhere to the roots of the plant.

It may indeed be ſaid, that in this caſe, the plant is capable of producing ſeed ſooner, not becauſe the bud contains the young plant more perfectly formed than the ſeed did, but becauſe it has more ſtrength, and becauſe it has one part completely formed, viz. the root. By this means it is better calculated for abſorbing from the ſoil the nouriſhment proper for it, and can digeſt more of it than a feeble root juſt formed from the ſeed is capable of doing. In anſwer to this objection, it may be obſerved, that the experiment ſucceeds equally well, and the plants

produce flowers and ſeeds as ſoon, when the ſimple bud is put into the earth, as when a quantity of root adheres to it. In many plants, however, this cannot be done; the bud and the root are ſo connected, that the one cannot be ſeparated without a part of the other adhering to it. But it can be done very perfectly in many plants, and particularly in all the bulbous plants. In theſe plants there is none of the root of the parent plant that adheres to the bulb, from which a new plant is to be produced. In the root of a tulip as it is commonly called, but which is really a bulb or germ, there is none of the root of the old plant adhering to it; it will however grow, and flower, and produce ſeed the year in which it is planted, while the ſeed of a tulip, upon being ſown, will produce a new plant, that will not bear ſeed ſooner than the ſixth year after the ſeed is ſown.

Though this experiment ſucceeds beſt with the plants which carry buds on their roots, it is not ſolely confined to them. In many trees the ſame thing takes place, though the buds adhere to the trunk and branches. In fruit trees it is well known that fruit reſembling that of the parent plant cannot be produced by merely ſowing the ſeed of the tree.

Thus the tree may be propagated by making a bud of it grow upon the root of another plant, by inoculating or grafting on it. It frequently happens in grafting particularly, that the bud produces flowers and fruit the very firſt year of its growth. This however, thoſe who are employed in culti-

vating fruit trees take pains to diſcourage, as the languid vegetation which would ſucceed the ripening of the fruit, would be hurtful to the bud in this artificial manner of making it grow. That this haſty formation of flowers and ſeed from a bud by grafting, in one year, while it would have required many years before this could have been effected in a plant raiſed from the ſeed, is not owing to the ſtrength or vigour of the roots of the ſtock upon which the bud is made to grow, is evident from our being frequently able to accompliſh the ſame end without the aſſiſtance of the root and trunk of another plant, by making the bud ſtrike root, and grow immediately without any aſſiſtance. This, however, cannot in fruit trees be effected, that I know of, by cutting off the bud and planting it in the earth. But what cannot be done in this manner directly, can frequently be done by bending a branch of the parent tree with buds on it, till it touches the ground, and then covering the branch with earth. The bud will in this ſituation unfold itſelf, produce a plant exactly ſimilar to the one it was produced from, in the ſame manner as if it had been grafted, and will produce flowers and fruit in a much ſhorter time, than a plant raiſed from a ſeed of the ſame tree could have done; and this happens though the connection betwixt the bud and the original plant be cut off the moment the bud is expanded.

The buds of the perennial plants, and the ſeeds of annual plants, agree much better in the time re-

quiſite for the propagation of the ſpecies than the ſeeds do: and there ſeems to be ſome reaſon to conclude that the ſeed of a perennial plant is leſs perfect, is ſimpler, and fitted for producing a leſs perfect plant than the ſeed of an annual one. That this really is the caſe ſeems evident, by attending to the ſtructure of the ſeeds more minutely, where it will be often found, that there is a much more ſtriking reſemblance betwixt the ſeeds of the annual plants and buds of the perennial, than there is betwixt the ſeed of the two kinds of plants. The ſeeds of all plants contain the rudiments of the plants to be produced. Sometimes the ſeed conſiſts entirely of this part, which is commonly known by the name of corculum or germ of the plant. In other plants, beſides the germ of the future plant, there is another part cloſely connected with it, known by the name of the cotyledons or lobes of the ſeed. Whether the ſeed of the plant conſiſts only of the germ, or of the germ and lobes connected with it, in both caſes they are concluded in a number of coats, which, ſo far as has been diſcovered, are inorganical and incapable of corruption. The germ is properly the ſeed of the plant, the other parts are uſeful during the infant ſtate of the plant, but form no part of it. It is only in the ſeeds of one claſs of plants, the moſſes, that we meet with a naked germ. In the ſeeds of all other plants, beſides the germ, there are cotyledons or lobes united with the germ. In ſome plants, there is only one

lobe; in moſt plants there are two; in a few there are four, ſix, or ten lobes.

When we examine the germ in the ſeed of plants, we ſee that it is compoſed of two parts; the one external, which is to become the root of the plant, and called radicula, or radicle; the other more internal, and buried in the lobes of the ſeed, or lodged between the lobes, when there are more than one, the plumula or plume, which, after extending, becomes the ſtalk or leaves of the plant. The radicle is always of a conical figure without any diviſions. The plume has no regular figure, and is divided into a great number of ſmall parts. When the ſeed beſides the germ conſiſts of lobes, the radicle ſends off a branch to each of them, which is ramified through them, and forms what is called the ſeminal root of the plant. When the plume is viewed with the microſcope it appears to be compoſed of a number of parts ſhaped like the leaves of the future parent plant, and requiring only to be a little larger to reſemble them exactly. An author who has written lately on the ſtructure of the ſeeds of plants, but whoſe name I cannot at preſent recollect, mentions ſome curious obſervations on the ſeeds of plants; and from his obſervations, as far as I remember, it appeared that in ſome plants the plume, viewed with the microſcope, contained not only the parts to become the ſtalk and leaf of the plant, but that frequently the flower of

the plant could be perceived in it with all its petals and ſtamina. I remember further that in the inſtances where this happens the ſeeds were all the ſeeds of annual plants, and that nothing of this kind has been obſerved in the ſeeds of perennial plants.

This, I think, in ſome caſes at leaſt, ſhews that the reaſon why ſome plants propagate their ſpecies in a ſhort time, is, that in the ſeeds all the parts for this purpoſe are already completely formed, and require only to be increaſed in ſize, while in the ſeeds of thoſe plants which require a long time before they flower and produce ſeed, the plume contains only ſuch parts as are to become leaves; the production of the flower is left to the young plant itſelf, which in time it produces in folds in the form of buds at the end of the ſummer preceding the ſpring in which they are to make their appearance in open day, and to propagate the ſpecies.

Though, however, there ſeems, from the foregoing obſervations, to be ſome reaſon to believe that there is a difference betwixt the ſeeds of annual and perennial plants in the formation of the germ, in conſequence of which one propagates the ſpecies ſooner than the other, we cannot conclude that this is the caſe in all. In many plants that are annual, the preſence of the young flower in the ſeed has not been diſcovered, partly perhaps from its ſmallneſs,

and partly from its being enveloped or concealed by the other parts of the ſeed. There may likewiſe be conſiderable differences in the ſtructure of the plume of perennial plants, eſpecially as we find that they differ remarkably in the time which they muſt grow before they produce flowers and ſeeds; thoſe that require the ſhorteſt time may have their ſeeds conſtructed like the annual ones; but no obſervations of this kind have been made that I know of; and perhaps thoſe plants, whoſe ſeeds moſt nearly reſemble the ſeeds of certain annuals, will be found in the circumſtances of the vegetation to approach nearer to the annual plants than thoſe perennials which differ more in that reſpect. They probably will be found not only to propagate the ſpecies ſooner, but for a leſs number of years than the reſt of the plants of the ſame claſs do.

Another difference in the ſeeds of plants is that ſome of them are capable of drawing their nouriſhment from the ſoil, and preparing it for all the purpoſes of vegetation the moment they are buried in the earth; while in other plants the ſeed when ſown, is ſome time before it is capable of doing this. In thoſe plants which conſiſt of a naked germ, the moment they are ſown the radicle becomes a root, and draws that part immediately from the ſoil which is for its ſupport, without requiring any aſſiſtance, or any other nouriſhment than what the ſoil immediately furniſhes. The ſeeds of all the moſſes are of this kind. They are ranked among the moſt im-

perfect vegetables, and require leſs nouriſhment, and that of a weaker kind, and their young is more hardy than the young plants of the more perfect kinds of vegetables. In the ſeeds of the more perfect plants it is ſome time before the radicle is capable of collecting that nouriſhment from the ſoil which is requiſite for the ſupport of the plant. The nouriſhment too which the ſoil contains, or which is contained in the other bodies which ſurround it, is of too coarſe a kind for the ſupport of the tender plant, or it cannot prepare it properly. In theſe plants, beſides the germ there are the cotyledons or lobes of the ſeed, which aſſiſt the radicle and plume in the firſt period of their growth, partly by abſorbing from the ſoil that nouriſhment which is beſt fitted for the plant, and tranſmitting it through the ſeminal root to the radicle and plume; but more eſpecially by a quantity of food prepared by the parent plant, of which, beſides the ſeminal roots, the lobes of the ſeed are almoſt intirely compoſed. This ſubſtance in the lobes is ſometimes intirely farinaceous, in others partly farinaceous and partly of an oily nature, and in others farina blended with other ſubſtances. It is intirely farinaceous in the common kinds of grain, as wheat, barley, and oats. In theſe plants there is only one lobe, and the plume and radicle are lodged in a hole at one end of it. In the almond, the poppy, and many other plants, there are two lobes, which conſiſt partly of farina and partly of oil.

The matter which the lobes of the ſeed contain is depoſited there, to ſerve for the nouriſhment and growth of the germ of the plant in the firſt days of its vegetation. It is a nouriſhment ready prepared, and already digeſted by the parent plant for the ſupport of the young plant which it is to produce. It is the aliment of their earlieſt age, while they are yet too weak to extract from the earth and other elements the materials which they are to change by their organic action into their own ſubſtance or juice peculiar to them. In the ſame manner as the yolk of an egg ſerves for the nouriſhment of the chick, while it remains in the ſhell; and as the milk of animals furniſhes to their young a nouriſhment already half aſſimilated, the digeſtion of which is eaſy, and proportionable to the weakneſs of the digeſtive organs. When the ſeeds of theſe plants are ſown which have nouriſhment already prepared for them in the lobes by the parent plant, the moiſture of the earth pervades the coats with which all ſeeds are covered, its impurities are by this means ſeparated, and it is abſorbed by the lobes more gradually; the matter in the lobes ſwells with the moiſture, a part of the nouriſhment in them paſſes into the ſeminal root, and is tranſmitted to the radicle and plume of the plant, which begins immediately to increaſe in ſize. Firſt the radicle extends itſelf, and then the plume. During this period the ſeminal root exerts ſome aſſimilating power, and the nouriſhment in the lobes changes its ſenſible qualities till it is ex-

hauſted in nouriſhing the young plant. The radicle and plume having by this time acquired ſufficient vegetative power and ſtrength, can prepare for themſelves from the earth and other elements the nouriſhment and juices fit for their ſupport and growth. The lobes having thus for ſome time ſerved as a root to the young plant, are no longer fit for this purpoſe. They fall off and decay, or begin to increaſe in ſize, and having for ſome time ſerved as root, till the radicle became ſufficiently ſtrong, they now become leaves, and aſſiſt the plume till its parts are ſufficiently unfolded, when they fall off and wither, having ſerved all the purpoſes for which they were intended.

ESSAY VII.

ON THE ROOTS OF PLANTS.

In a former eſſay it was obſerved, that the ſeeds of all plants conſiſted principally of the rudiments of the root and ſtem of the future plant: that the ſeeds of one claſs of plants conſiſted ſolely of thoſe parts; they are capable of drawing directly from the ſoil the part neceſſary for their ſupport. The plants thus conſtructed are of the leſs perfect kind; they never grow to a great ſize, and their nouriſhment ſeems to require but little preparation. They accordingly ſubſiſt upon the moſt barren parts of this earth: and ſerve principally to produce in time a ſufficient thickneſs of ſoil, for the nouriſhment of the more perfect vegetables. The ſeeds of the moſt perfect plants contain, beſides the rudiment of the root and ſtem, a part containing nouriſhment for the young plant, intended to ſupport it, till its own root be capable of drawing from the earth nouriſhment proper for it. During this time the young

root increaſes in ſize, but is not employed in nouriſhing the plant. At firſt it has a ſtrong diſpoſition to ſink into the earth, ſo ſtrong that it cannot be prevented. If one turn its end upwards, and forcibly keep it in that direction, it will bend ſo as to deſcend, while the young ſtem will bend in an oppoſite direction; and this will happen in whatever ſituation the ſeed may be, and equally in a dark as in a light place.

The root and ſtem of the plant ſeem in their infant ſtate to differ more remarkably from each other than at any other period of their growth. In old plants it frequently happens that the root will become the ſtem, and the ſtem the root. In proof of which it may be ſufficient to obſerve, that in raiſing plants from cuttings it is of little conſequence which end is uppermoſt; the roots will ſpring from either end, and if the roots of a plant be dug up, and the ſtem planted in the ground, the ſtem will ſend forth roots, and the old root will ſometimes get leaves, and in this way the plant will grow for a long time. But in this unnatural poſition it is not ſo ſtrong or vigorous. The branches which iſſue from it are long and ſlender, and hang to the ground and in this manner, it is ſaid, that weeping trees of moſt kinds may be produced. This length and ſlenderneſs of branches is, however, only a proof of the imperfect nouriſhment which the plant draws from its new roots. The plants thus raiſed are not only leſs vigorous, but ſhorter lived than theſe that

are ſupported by the original root. In the young root and ſtem this alteration in function is impoſſible; it never takes place till after the plant is a year old, and generally ſucceeds better if a part of it is two, and the reſt one year old.

The root, as it iſſues from the ſeed, is generally a ſingle fibre. Sometimes two or three fibres iſſue together, but they do not communicate with each other. At firſt they increaſe in length without becoming ſenſibly thicker, and this increaſe is made almoſt intirely at the extremity, the parts near the ſeed extend very little. This can eaſily be ſeen by making a ſeed grow in a bit of ſponge, or cotton, or moſs, on the top of a glaſs of water. In this ſituation the original root will extend itſelf frequently till it fills the glaſs intirely, and will make many revolutions round the inſide of the glaſs. The roots in this ſtate are very white and gloſſy, and make a very beautiful appearance. If the end of one of the fibres is cut off with a pair of ſciſſars the growth in length is inſtantly ſtopped and this will happen though the quantity cut off be not the hundredth part of an inch. In a few days a number of little tubercles will be formed on the ſurface of the cut fibre, from which will iſſue a number of branches though no branches will be formed on the uncut fibres, nor probably ever would be formed, unleſs the extention in length were prevented. When the roots of a tree get into a quantity of running or ſtagnating water the fibres are all ſingle, and extend to a great length. If the

growth in length is prevented, lateral branches will iſſue.

This growth in length may be prevented naturally by the extremity of the young root being cut off by an inſect, or more commonly by its meeting with ſome hard matter in its courſe, which it cannot penetrate, and cannot get round without being put very much out of its natural direction. In this caſe it ceaſes to grow longer, and a number of lateral branches immediately iſſue. There are few ſoils ſo very ſoft and uniform but ſuch obſtructions may be met with, and in conſequence of this the roots of plants conſiſt of a great number of branches. It is however, neceſſary in moſt plants that the roots ſhould extend to a conſiderable diſtance from the ſtem of the plant in a horizontal direction, becauſe the earth far below the ſurface has little power of nouriſhing plants. If the ſoil, therefore, is very hard, though numerous branches will iſſue from the roots, yet they will not reach to a great diſtance, and proper nouriſhment will not be procured. This will principally happen, when the roots are weak and delicate, but it likewiſe happens in thoſe that are moſt woody and robuſt, and which require the ſmalleſt nouriſhment. It is ſurpriſing to ſee how far roots will extend, and what obſtacles they will overcome in order to reach that ſoil which is proper for their ſupport.

When the young root has grown to a certain length, but particularly when it begins to

ſend off lateral branches, the nouriſhment contained in the ſeed, and which was prepared by the parent plant, is nearly exhauſted. That part, therefore, falls off, or by a little alteration becomes a ſort of leaf, and ſupplies the place of theſe parts till at laſt it falls off altogether. When the nouriſhment of this ſecondary part of the ſeed is exhauſted, the root begins to prepare nouriſhment for the plant. At firſt the quantity neceſſary is but ſmall, as the ſize of the plant above ground is trifling. As this increaſes, the root extends itſelf to be able to collect and prepare proper nouriſhment, and to give that ſtability to the part above ground, and even that direction to it which is moſt proper for the particular plant. Hence the diſtance to which the root extends, and the ſtrength which its parts acquire, neceſſarily depend on the height and ſize of the part above ground.

The parts in all plants that imbibe moiſture are probably the ſmall and almoſt inviſible portions of the roots. The thick fibres are probably intended to give them ſupport, or perhaps may be otherwiſe neceſſary in preparing thoſe fluids imbibed by the capillary fibres of the root. When a plant is dug up with great care, none of the large fibres are deſtroyed, yet the growth of the plant is always very much retarded by it, and very frequently altogether deſtroyed.

The organization by which the roots of plants draw nouriſhment from the ſoil, is the ſame in all plants.

The diſcoveries made in the ſtructure of the roots of plants have not hitherto enabled us to explain their growth, any more than the anatomy of animals has enabled us to explain their functions; both are ſtill out of our reach. But it is to have proceeded one ſtep in the diſcovery to know the organs by which this is done. The manner in which they operate may, perhaps, be ſome other time explained to us. We know that the roots of plants conſiſt of a great number of veſſels, moſt curiouſly interwoven, that beauty and elegance is given to theſe veſſels by means of a ſoft ſpungy ſubſtance, the pith, which is contained in the centre of the root, and readily accompanies every production from it; and that the whole is covered by a fine membrane like the ſcarf-ſkin of animals, which defends the reſt, and ſerves as a ſtrainer to the juices abſorbed by the vaſcular part of the root. This ſtructure, however, has not unfolded to us, why a vegetable has life without voluntary motion. But theſe organs are neceſſary; if any of them are taken away or injured, the growth of the plant is injured or deſtroyed.

The ſubſtance abſorbed by the roots is the ſame in all plants; the quantity of nouriſhment neceſſary for their ſupport is different in different plants. The firſt is evident from chemical experiments which ſhew that they are compoſed of the ſame principles, and Dr. Hales and Mr. Bonnet have proved that plants abſorb from the ſoil only water and air, at leaſt that water and air will ſupport plants equally

well with the ſoil in which they naturally grow. That the quantity of nouriſhment or rather the readineſs with which it is adminiſtered is different in different plants is evident from the different ſituations in which they naturally grow. Every part of this earth has plants adapted to it. The rock of a volcano, the richeſt ſoil, the field, the marſh, the moſs, the pond, the river, the ſea, have plants peculiar to them. In theſe different ſituations water is furniſhed more or leſs plentifully, and a difference in the ſhape of the part for abſorbing that moiſture becomes neceſſary. The ſize a plant grows to, the poſition it requires to be placed in, the time that it grows, is different in different caſes, and an alteration in the root, the principal part in plants, becomes on theſe accounts neceſſary.

The age of a plant depends intirely on its root: ſome live one year, ſome two years; others many years. In thoſe plants that live only one year it is rarely that they acquire any remarkable ſize, they need, therefore, but little ſupport to keep them in a proper poſition, and a ſmall quantity of nouriſhment. The root is in them ſmall, and there is no reaſon for its ſpreading to any great diſtance, or becoming ſtrong and woody. The roots of the annual plants conſiſt generally of a few fine fibres, which ſink only a little way into the ground. The ſame obſervation is applicable to the biennial plants; they are never large, and require but little ſupport or nouriſhment; and it is principally by alterations in the

figure of their roots, that they are fitted for different ſoils.

It is in theſe plants, that laſt for many years, that the greateſt diverſity of roots is obſervable. As the number of years they are diſpoſed to laſt is very different, they differ greatly in ſize, and their roots vary prodigiouſly from each other, in the direction in which they ſink into the earth. When the roots of plants are ſaid to differ from each other, it ought to be underſtood to be meant to extend only to thoſe parts in the roots of plants, that are employed in nouriſhing the plant. The parts that are occaſionally attached to roots, ſuch as bulbs, or gems, as they have no ſhare in nouriſhing the plant, and as they are deſtined ſolely for the nouriſhing of the leaves and fruit of other years, ſhall not at preſent be conſidered.

The moſt remarkable circumſtances, in which the roots of plants differ, are ſize, figure, and direction, the laſt of which is the cauſe of the greateſt differences among them. In ſome caſes, the root conſiſts of a great number of ſmall fibres, which ſpread near the ſurface of the ground, and form an angle with it from 20 to 30 degrees. In theſe we ſeldom find, that they ſink above a few inches into the ground, and never below the vegetable mould, and in the largeſt trees not above a foot or two. Of this kind are the fibrous and the branching roots of plants, which two do not differ from each other excepting in ſize. The fibrous root is ſlender

and weak, ſufficient to ſupport the ſmaller delicate plants. The branching root is deſtined to nouriſh the larger and more robuſt plants. When any injury is done to theſe roots, we ſeldom find that the plant is injured much. The wounded part, as it is always ſmall, ſoon heals up, and if a part happens to be cut off, new branches ſpring, and ſupply its place. Though this ſhould be done two or three times, the plant not only ſtill ſurvives, but grows with vigour. The only thing injurious to the roots of plants of this kind is ſhaking them, by which their hold of the ground is diminiſhed, and the more tender fibres, by which the nouriſhment is chiefly abſorbed, are deſtroyed.

This, frequently repeated, checks the growth of the plant, and, in time, ſtops it altogether. Another circumſtance, which would be perhaps injurious to the roots of the fibrous and branching kind, is, the forcing them much out of their natural direction, and obliging them to ſink deep into the earth. Naturally, they ſink but a ſmall way into the ground, they require, of conſequence, a rich ſoil for their ſupport. If they are, any how, made to ſink deeper into the ſoil than is natural to them, their growth is hurt, partly by the greater preſſure of earth upon them, partly by their being farther removed from the ſoil near the ſurface, which, from their natural direction ſeems to contain the moſt proper food for them. If roots, therefore, of this kind, are not planted too deep, and if the plant is in ſuch a ſitu-

ation, as not to be ſhaken by the wind, it will not be injured by being removed.

What has been ſaid of theſe roots is applicable in a ſtill greater degree to thoſe which are called creeping. This ſpecies of root extends parallel to the ſurface of the ground, and ſend forth branches at equal diſtances. The roots of this kind are viviparous, and are not only capable of ſupplying thoſe roots that accident may have deſtroyed, but even of ſupplying the loſs of the ſtem, and leaves of the plant, and thus propagating the ſpecies, without the aſſiſtance of flowers and ſeed. Theſe new ſtems are called ſuckers. It is commonly obſerved of this ſpecies of roots, that the viviparous power increaſes, according to the diſtance this plant is from the ſeed. So that the root produced from the ſeed it is imagined, has this power of ſending forth ſuckers in a leſs degree than a ſucker has, and the ſucker from a ſecond ſucker is ſuppoſed to have this power in a greater degree than the firſt. This notion, though inculcated very much by eminent writers, does not ſeem to be ſufficiently ſupported by experiment. So far as I have had an opportunity of attending to the growth of plants diſpoſed to ſend out ſuckers, I never obſerved theſe ſuckers more diſpoſed to ſend out ſuckers than the ſeedling. But I always have obſerved, that the plants thus produced are leſs vigorous and leſs beautiful than the ſeedlings. The creeping root, in conſequence of this viviparous quality, bears to be altered more

with regard to its depth below ground, and is leſs liable to be ſhaken or hurt by the wind. They bear removing better, when planted ſingle, than plants with fibrous roots.

In the two kinds of roots hitherto mentioned, the direction of the whole is either horizontal, or inclined at a ſmall angle to the horizon. The roots are, therefore, very near the ſurface; the nutriment in the ſoil is almoſt all taken up by theſe roots; they require rich ſoil, and injure the graſs in their neighbourhood. The laſt ſpecies of root I ſhall mention is the ſpindle ſhaped root, the carrot root as it is frequently called. This differs both in its figure and direction from thoſe hitherto mentioned. Its figure is that of an inverted cone, terminating in a point, from the whole ſurface of which, there iſſues at right angles to it a prodigious number of ſmall fibres, that ſpread to it from a conſiderable diſtance. The ſize of theſe fibres bears but a ſmall proportion to the ſize of the root from which they ſpring. The larger the ſpindle-ſhaped part is, the greater number of thoſe ſmall fibres iſſue from it. But the principal fibres ſeem to be incapable of further ſubdiviſion, at leaſt if they are, the diviſion is not capable of being carried ſo far as in other roots. This root ſinks perpendicularly into the ſoil, the branches from it ſpread horizontally, and it is the only kind of root in which it may be juſtly ſaid that the root ſinks as deep into the ſoil as the ſtem riſes above it. In this root, the fibres ſhoot as

a great depth, far below what may be called vegetable mould. If the plant be of any conſiderable ſize above ground, it ſinks into and ſpreads among the regular ſtrata of earth. The nouriſhment it draws is not ſo rich and ſubſtantial, as that contained in the ſoil at the ſurface, as is evident from their growth being more checked by cutting and dreſſing, than ſuperficial roots. The ſpindle root is a ſingle fibre, which lengthens only at its extremity, like all roots, and ſinks perpendicularly into the earth. The ſurface which this root expoſes to the ſoil is not to be reckoned from that of the principal fibre. That is too ſmall in large trees to be ſufficient for the ſupport of the plant; but it muſt be reckoned from the fibres, which, riſing from this trunk, at right angles from it, ſpread in a horizontal direction. According to the depth of the root, whence they ſpring, they will draw a richer or a poorer nouriſhment, and theſe being afterwards mixed, produce a compound fit for the ſupport of the plant. As the greateſt part of the roots of this kind are far below the ſurface, the ſoil at the ſurface will be leſs impoveriſhed, and be fitter for the ſupport of plants, whoſe roots are of a different ſhape. In roots of this kind, if the end of the perpendicular fibre is cut off, or injured by an inſect, or prevented from following its natural direction by the hardneſs of the ſoil, its growth in length will be ſtopped, and, of conſequence, it will not be able to ſink deep enough into the earth

to draw the proper nouriſhment for a full-grown plant: becauſe the root will not grow to ſuch a ſize as to afford ſurface for as many roots to ſpring, as are ſufficient for drawing nouriſhment to a large plant. This, at firſt, may not perhaps, apparently hinder the growth of the plant; for a while, it may ſhoot with ſeeming vigour; but if the plant is of that kind, that naturally grows to a great ſize, it will in time be obſerved, and the plant, upon whoſe root this injury has been committed, will never grow to the ſame ſize, or live to the ſame age which it would otherwiſe have done.

Theſe obſervations on the roots of plants may be illuſtrated and confirmed, from the culture of all perennial plants, but is moſt evidently confirmed from the growth of trees, which, having a woody ſtem, grow to a great ſize, and afford time for obſervation, which cannot be had in ſhort-lived plants.

Among the trees, the aſh, elm, horn-beam, and ſycamore, have fibrous, or branching roots. The poplars are the moſt remarkable trees with creeping roots. The oak, cheſtnut, walnut, and beech, are the moſt remarkable trees with ſpindle-ſhaped roots. The aſh is the moſt common tree with fibrous or branching roots. They ſpread to a great diſtance, and ſink into the ground at an angle of 20 degrees, near the ſurface of the earth, impoveriſhing the ground to a great diſtance; ſo that the corn-fields in the neighbourhood of the aſh are leſs fertile than the ſame ſoil at a diſtance from them. In the farms in the coun-

ties of Glouceſter and Cheſhire, they will not allow any aſh trees to grow. They ſay, the cattle eating the leaves have the qualities of their milk ſpoiled: but it is likewiſe owing to the aſh-roots impoveriſhing the ſoil, and ſpoiling the graſs. It is a common obſervation, that the drop of the aſh tree is more injurious than that of any tree. This, however, I do not imagine ariſes ſolely from the bad quality of the rain which falls from its leaf; for I have ſteeped aſh leaves in water, and uſed it in watering young trees and other plants, without finding that they were injured by this treatment. In digging up aſh trees, the moſt of the roots are obſerved to ſpread near the ſurface. In conſequence of this it thrives in a ſhallow ſoil. It bears tranſplanting well, but is improper for hedge-rows, from its impoveriſhing the ſoil in its neighbourhood. The wood is valuable, but it requires rich land to make it grow with vigour. It is of ſlow growth, and is not ornamental, from its being ſo late in the ſeaſon before it gets its leaves.

The ſycamore or plane tree has roots like the aſh, but they ſink into the ground at a greater angle, and go deeper than the aſh. It does not injure graſs, is equally eaſy tranſplanted with it; grows like it, beſt in rich ground, but requires a little little lighter ſoil, and as it gets better hold of the earth, it grows in open fields more eaſily, but thrives ſtill better in groves.

Pretty much of the ſame nature is the elm, but

its roots are more of the creeping kind, ſink deeper into the ſoil than the aſh, yet will grow very well in a clay ſoil, and is excellent for hedge rows. All cattle, horſes, ſheep, &c. are fond of its leaves, and it bears tranſplanting very well. The horn-beam has the ſame kind of root, likes poor ſtiff clay, is eaſily tranſplanted, but injures the graſs. It is a valuable wood, and it is ſurpriſing it is not more cultivated. The wood burns like a candle and is very white and tough, and fit for many purpoſes in huſbandry.

All theſe trees have fibrous and branching roots. The plane ſinks the deepeſt, and the horn-beam the leaſt into the ground. They all thrive in a ſhallow ſoil, with clay or tilly bottom, which prevents the root from ſinking deep. They require to be ſheltered from the winds to hinder their roots from being looſened, and grow beſt in groves. None of the roots ſink into the ground at a greater angle than one of 35 degrees; they all bear tranſplanting well, almoſt at any age, and can then only be injured by planting their roots too deep. The ſame obſervations apply to the poplars, only as they have creeping roots, they bear tranſplanting ſtill better, and, provided they have plenty of moiſture, thrive well in any ſoil, but worſt in clay: their roots ſpread near the ſurface, impoveriſh land, and will allow nothing elſe to grow.

The oak, cheſtnut, walnut, and beech, have ſpindle-

ſhaped roots. They require a deep ſoil. They do not bear tranſplanting, and we never find them to grow to their full ſize, but when in natural woods, where their ſeeds have been ſown in places where they are now growing. When raiſed in a nurſery, and tranſplanted, they thrive for ſome time, make a tolerable ſhew as ornamental trees, but are ſeldom ſtraight, tall, and beautiful. They always retain the marks of this improper management, and and are generally crooked and of low ſtature. All the trees that bear nuts are furniſhed with the ſame roots, and are injured by the ſame treatment. The fir tribe, in which I include the larix and cedar, may be called nut-bearing trees; for they are all furniſhed with a hard ſhell, which includes a number of ſeeds. They have roots ſomewhat ſingularly ſhaped. The principal root reſembles a ſpindle. This ſinks perpendicularly into the ground, and they require, accordingly, a deep gravelly ſoil, and do not bear tranſplanting. The leaſt injury to the extremity of this root checks the growth of the tree. But they have likewiſe a great number of ſmall roots that ſpread horizontally, almoſt even with the ſurface of the ground. In conſequence of this they deſtroy the graſs in their neighbourhood, and though they do not die when their ſpindle root is inert, they do not grow to a great ſize, are unhealthy, and are capable of dragging out a miſerable exiſtence for a great length of time.

Theſe obſervations on the roots of trees point out the reaſon why natural woods excel thoſe that are planted, becauſe in all caſes the roots of trees are injured by tranſplanting; why the injury is greateſt to the roots of thoſe trees that ſink deep into the ground; why the aſh, the elm, the hornbeam, and even the ſycamore may be raiſed in the nurſery, in the uſual manner; and why no attention is neceſſary in the raiſing of poplars, but to plant them in a wet ſoil. But if we wiſh to raiſe thoſe moſt uſeful and beautiful of all trees, the oak, the cheſtnut, the beech, and the walnut, we muſt chooſe a deep ſoil, and plant the ſeeds where the trees are intended to remain; all the trouble this requires is but keeping them clear of weeds the firſt year or two. The cheſtnut and walnut may thus be reſtored to this country; and thoſe who may diſlike this manner of rearing trees, as being ſlower than by planting large plants from a nurſery, may be aſſured, that any difference of this kind will not be obſervable in three or four years. This practice might be extended to advantage perhaps in the fir tribe; though in them, planting them two or three years old from the nurſery will anſwer very well. If the cheſtnut and walnut are raiſed for the ſake of their nuts, the practice recommended is not proper, it is only applicable when we raiſe them on account of their wood: but this is ſo obvious that I ſhall not take up time by inſiſting on it.

Neither do the obſervations apply where large ſhoots

are wanted, where length is of more importance than any thing, as in ſhoots for hoops, &c. For this purpoſe a different ſoil from that mentioned above is neceſſary, ſuch as contains much moiſture, and is eaſily penetrated. Thus the aſh in boggy land makes large ſhoots fit for hoops, and is very profitable.

ESSAY VIII.

ON FERMENTATION.

Read A. D. 1785.

A BODY is ſaid to have been fermented when its ſenſible qualities are much changed without any remarkable cauſe of the alteration being perceived. All vegetable and animal ſubſtances are capable of having their properties more or leſs altered in this manner, unleſs they have been expoſed to ſuch a degree of heat as to diſſipate their watery moiſture either wholly or partially, and reduce them to the ſtate of charcoal, or dryneſs. They alſo may be deprived of their diſpoſition to ferment by the addition of certain ſubſtances of the ſaline, bitter, or aromatic kind. This change in the qualities of bodies is often the only mark of preceding fermentation; though ſometimes it is attended through its whole courſe, with a hiſſing noiſe, the emiſſion of

numerous air bubbles, and a copious froth or ſcum upon the ſurface.

Though all vegetable and animal ſubſtances in their recent or freſh ſtate are capable of having their properties changed by fermentation, yet the change in quality as well as the circumſtances which accompany it, vary much in different individuals. We always find that animal and vegetable ſubſtances mixed with much water, fall very readily of themſelves into this proceſs. Frequently the eye can diſcover no change in the properties from which the preceding fermentation can be inferred; yet the alteration in conſiſtence, in taſte, or ſmell afford a certain method of diſcovering fermentation. In many caſes, however, we can perceive that ſomething is going on, and from the quantity of elaſtic matter ſeparating, we are led by experience to expect a change in the other qualities of the body. Theſe changes, whether induced by inviſible or ſenſible fermentation, have given origin to the different ſtages into which fermentation has been divided. A body, by fermentation, is ſometimes changed into a vinous liquor, of the nature of wine or ale; in others it acquires acid properties like vinegar, with or without the preceding vinous ſtate; and in others the body becomes putrid and rotten after having been in the ſtate of wine or vinegar, and often becomes putrid without having ever been in any of theſe ſtates. Hence fermentation has been divided into three ſtages, the vinous, the acetous, and the

putrefactive. All ſubſtances that are inflammable and capable of mixing with water may be made to ferment in ſome of theſe three ways.

As all bodies that ferment are inflammable, ſo we find generally that more or leſs heat is produced during the proceſs. The more heat that is produced by the fermentation, the more nearly does the body approach in its properties after it to the ſtate it would have been left in after inflammation. To what this is owing is not very eaſily aſſigned, but one is almoſt led to imagine, that air, which is the principal agent in producing heat during the inflammation of a body, is alſo in part concerned in producing the heat in fermentation, and is likewiſe the chief cauſe of the change in the properties of bodies produced by it. Some of the changes produced by fermentation are uſeful or agreeable. We therefore encourage the proceſs in various ways. When the change is diſagreeable or hurtful, we endeavour as much as poſſible to prevent it. Though theſe changes have been long known, and though the induſtry of man has been long employed to promote or retard them, the cauſe of them is not yet properly aſcertained. Many explanations of them have been given by eminent men, and much has been ſaid upon the ſubject. It is not my intention at preſent to give a full account of fermentation: it would take up too much time, and would furniſh matter for ſeveral eſſays. I ſhall confine myſelf at preſent to the cauſes of the differences obſerved in theſe bo-

dies, which have undergone the vinous fermentation. It may be proper to obſerve, that great differences in fermentation ariſe from the various ſpecies of matter of which different ſubſtances are compoſed. According as the body is ſimple or compounded its fermentation will conſiſt of only one ſtage, or of two or three joined together. Of all the parts of vegetable or animal ſubſtances which may be fermented, thoſe that have a ſweet taſte are capable of being changed in the greateſt variety of ways by it. If the ſweet matter is immixed with any other, we can make it firſt into wine, then vinegar, and laſtly putrid; and the order cannot be altered or reverſed. It cannot be made into vinegar after being putrid, or into wine after being vinegar.

Vegetable ſubſtances furniſh this ſweet matter in greateſt abundance; a little ſweet matter is alſo found in the animal kingdom, but only in the milk of animals. The milk of animals in its fermentation reſembles that of ſweet vegetable matter, but on account of its intimate mixture with various animal ſubſtances, it differs in ſome reſpects from it. Nature has furniſhed a vaſt variety of vegetables that have a ſweet taſte; and as along with the ſweet matter there is a great quantity of water, the proceſs of fermentation is frequently going on ſpontaneouſly, and muſt have been obſerved at a very early period. Sweet ſubſtances are thoſe only that can be reduced into the ſtate of wine. The exhilarating quality

which the ſweet matter thus acquires, made it very generally liked wherever it was known, and the vinous fermentation was probably not only the firſt ſpecies of this proceſs performed intentionally, but likewiſe the firſt which became the buſineſs of a trade or profeſſion.

We do not know when this diſcovery was firſt made; but wine ſeems to have been a very early ſpecies of luxury: in many places it is a neceſſary of life. In thoſe countries where grapes were ſcarce, other fruits were ſubſtituted; and where fruit was not ſo plentiful, the farinaceous ſeeds prepared in a particular manner afforded a liquor poſſeſſing the intoxicating quality, though not the pleaſant taſte and flavour of wine. In countries where neither fruits nor grain were common, or where the manner of fermenting them was unknown, ſubſtitutes were diſcovered which poſſeſſed the enlivening or ſtupifying qualities of the juice of the grape. Some of theſe were prepared from milk by fermentation, and others are aromatic ſtupifying plants, ſuch as opium, tobacco, the areca, and beetle-nut, and the kava of the South Pacific Ocean. It has indeed been aſſerted that no liquor like that which Buchanan alleges the ancient Caledonians employed, can be procured from milk. But whey boiled to a proper conſiſtence may be made to fall into the vinous fermentation.* The ſame precautions are indeed requiſite, with-

* A liquor of this kind was uſed not only by the Gauls but alſo by ſome of the Tartar Nations.

out which it will become four, inſtead of acquiring the properties of a vinous liquor.

Of all the fruits, the vine is that whoſe ſweetneſs moſt eaſily falls into the ſtate of an agreeable vinous liquor by fermentation. But the wine from the juice of the grape has been known from a very remote period, and the ancient Greeks and Romans were good judges of it, and well ſkilled in its preparation. Yet they do not ſeem to have underſtood the change which the fermentation had produced upon the nature of the juice in the fruit. At leaſt we are entitled to conclude this from their total ſilence upon this ſubject.

At what period mankind made the diſcovery that wine owed its chief qualities to the production of a ſmall quantity of fluid, ſuch as is now called ſpirit of wine, is not more diſtinctly known. When it was firſt diſcovered it would be conſidered as very curious, that a ſmall quantity of a colourleſs fluid being ſeparated from wine, it became nauſeous and diſagreeable; while the fluid ſeparated, though it had little of the ſmell or flavour, and none of the taſte of the wine, poſſeſſed its other properties in a very high degree. The diſcovery ſeems not to have been made in the time of Pliny, for he mentions nothing about it. The only thing which looks like it in any ancient author, is the diſcovery aſcribed to the elder Cato, of ſeparating water from wine by putting it into a diſh made of dry wood. Wine and water mixed poured into a veſſel made

of this wood, were ſoon ſeparated through the pores of the wood, and the wine remained behind. An ancient author, giving an account of this diſcovery, ſays, that the wine paſſed through, and the water remained behind. Mr. Ray ſays, in his *Hiſtoria Plantarum*, that he tried the experiment, and that no ſeparation of the wine from the water, or water from the wine took place, but that both paſſed through the wood together. A modern traveller, indeed, aſſerts, that he aſtoniſhed ſome of the wandering Arabs with this experiment; and mentions it as a ſtrong mark of their ignorance, in being ſurpriſed at an experiment, which every body in Europe knew how to perform.

The Arabians are ſaid to have firſt diſcovered the method of ſeparating ſpirit from wine, but at what period is unknown. It is, however, not improbable that this was firſt diſcovered at the time when attempts were made to form gold and ſilver.

Subſtances which have undergone the vinous fermentation, furniſh a portion of ſpirit of wine, which, when properly purified, ſeems to be poſſeſſed of the ſame properties, though the ſubſtances which yielded it differ much from each other. The ſpirit at firſt obtained poſſeſſes ſome of the properties of the ſubſtance from which it was obtained, particularly a little of the original flavour, ſo that we can eaſily diſtinguiſh by the ſmell, and other properties of the ſpirit, the ſubſtance which furniſhed

it. But even when the differences betwixt ſpirits are ſtated as high as poſſible, they never equal the differences between the vinous liquors which furniſhed them.

The difference between wines and the ſpirit which they yield by diſtillation is owing to the manner of managing the fermentation, and allowing it either to proceed ſlowly or more rapidly, or ſtopping it altogether after it has proceeded ſome length. But conſiderable differences ariſe from the various ſubſtances with which, in different bodies, the ſweet matter is blended. It may, perhaps, not be improper to mention ſome inſtances of the effects of each of theſe, in the management of the ſweet matter of the grape.

The quantity of ſweet matter in the grape varies in the ſame ſpecies, in different years, and in different ſoils. In no caſe, does the juice of the grape conſiſt of mere ſweet matter and water: it always contains, beſides theſe, ſome farinaceous and mucilaginous matter, a ſmall quantity of odorous matter, ſome of the eſſential ſalt of the vine, commonly known by the name of tartar, and a little colouring matter. When the ſeaſon is favourable and the ſoil good, the reſt of the ingredients bear but a ſmall proportion to the ſaccharine part, and the quantity of water is alſo very little. But in bad ſoils, and unfavourable ſeaſons, the juice of the grape is very watery, and the ſweet matter is much diminiſhed. The proportion of the other parts is greater and more perceptible

to the taſte; the grape does not acquire that luſcious ſweetneſs, but in its riper ſtate has a rough and ſour taſte. When the juice has ſuch different qualities in different ſeaſons and ſituations, the liquor produced by fermentation is proportionably various. No part of the juice, excepting the ſweet part, can be changed into ſpirit by fermentation. The acid and farinaceous or mucilaginous parts are either advanced already to another ſtage of fermentation, the acetous, and cannot be made to return back again to the vinous, or are of ſuch a kind as to be incapable of both the vinous and acetous fermentation. Of this kind are the farinaceous and mucilaginous part, which neither can be made ſour nor vinous, if perfectly pure. The liquor produced in this caſe is more acid and ropy than it would have been if the proportion of ſweet matter had been greater.

But the quantity of acid and other parts in the wine are not thoſe only which originally exiſted in the juice of the grape: a part is produced by the fermentation; though the pure ſweet matter would all be changed by it into pure ſpirit, yet, ſweet matter being capable of becoming both ſpirit and vinegar, by fermentation, according to circumſtances, a part changes into the one, and a part into the other, if the fermentation is not ſtopped preciſely at the end of the vinous ſtage. This is not eaſily effected, even when the grape is in the moſt favourable ſtate. When it is not ſo, when it contains acid parts at

firſt, it is hardly practicable: for the acid part in the grape is very much diſpoſed to bring on the acetous fermentation from the beginning, and ſhould it fail of having this effect, it is ſtill diſpoſed to make the wine run into the acid ſtate immediately after the vinous fermentation is over.

The farinaceous and mucilaginous parts alſo have a ſhare in producing this effect. In their pure ſtate, they can neither be changed into wine nor vinegar by fermentation, as has been obſerved a little ago. Notwithſtanding this, when mixed with a little ſweet matter, they all ferment together, and may either change wholly into wine, or wholly into vinegar, according to the proportion of ſweet matter mixed with them. If the quantity of ſweet matter be very great, the farinaceous parts are entirely changed into the ſame wine, with the ſweet part. If the quantity of ſaccharine matter is very ſmall, the whole becomes vinegar, and has little appearance of ever having been in the vinous ſtate. The mucilaginous part is diſpoſed to alter a little in the ſame manner, though its effects are not ſo well underſtood, or ſo well confirmed by experiment.

This property of farinaceous matter mixed with ſaccharine matter has been long known; and it has been alſo long known, that farinaceous matter mixed with ſome other ſubſtances, is capable of undergoing ſtill more wonderful changes. Thus, a quantity of meal from wheat, barley, or oats, whoſe greateſt part is farinaceous, when mixed with water,

becomes, by fermentation, a little acid, but ſeems never to acquire any of the properties of wine, but remains in the ſtate of flummery. If the ſame meal be mixed with a quantity of ſugar, or any other ſweet matter, it then falls into the vinous ſtate, and the quantity of ſpirit produced is much greater than the ſweet matter alone would have furniſhed. We know, that the farinaceous matter, when it conſtituted a part of the ſeed of a plant, is capable of being changed intirely into ſweet matter. Moſt kinds of grain have little or no ſweet matter in them in their natural ſtate. When put into the earth, they gradually acquire a ſweet taſte, and retain this during the tender ſtate of the young plant. When the plant has acquired ſufficient ſtrength to ſupport itſelf, the ſweet taſte is no longer perceptible. The ſame change is produced upon the ſeed, when ſteeped in water, and laid in heaps to vegetate; and if the vegetation is ſtopped at a proper period, the whole farinaceous parts of the ſeed change into ſweet matter, which, like the native ſweets, may be made to undergo the vinous fermentation, and be changed into ſpirits. This artificial ſweet matter is in its greateſt perfection when the vegetation has proceeded ſo far that the plume has become as long as the ſeed, and begins to penetrate the ſcarfskin at the end oppoſite to that where the corculum is fixed to the cotyledon.

Many have thought, and ſtill think, that the powers of vegetation only can produce this change

in the farinaceous matter, and have been diſpoſed to conſider the growth of ſeeds and the generation of animals as being each the produce of fermentation, though of a different kind from any I mentioned in the former part of this eſſay. But whether we conſider this alteration in the farinaceous matter as a diſtinct ſpecies of fermentation or not, certainly the powers of vegetable life are no way concerned, or neceſſary to it. It is not during the growth only of the ſeed that this change can be effected; but a quantity of the ſweet matter produced by the growth of the ſeed mixed with a quantity of the ſame ſeed ground into powder, and the whole mixed with a proper quantity of water, will all become ſweet, and fall afterwards into the vinous fermentation, and be changed into ſpirits in the ſame manner as if the whole had been previouſly altered by the vegetation of the ſeed. Were it not for this property of the farina, great loſs would frequently be ſuſtained by the farmers in unfavourable ſeaſons; grain that has once begun to grow, and whoſe vegetation has been ſtopped, can never be made to grow again. Such grain never can undergo any further malting; when grain has been made to grow in this improper manner, it can hardly be ſuppoſed that the change into ſaccharine matters is perfect or complete. It, therefore, would be leſs proper for the vinous fermentation, and would furniſh a ſmaller quantity of ſpirit than grain which had been perfectly malted. This grain, however, when mixed with a quantity of

perfect malt, and fermented, furniſhes as much ſpirit as if the whole had been in the ſtate of malt. The perſons in this trade even prefer it to an equal quantity of malt; for in good ſeaſons, when no ſuch half malted or half ſpoiled grain can be got, they take good grain, reduce it to meal, and mix it with their malt, and are ſatisfied that they obtain more ſpirits in this way, than from an equal quantity of good malt. Thoſe who follow this practice are very good judges of this circumſtance, and indeed, it is otherwiſe ſo eaſily aſcertained, that we can hardly ſuppoſe them miſtaken. If one was diſpoſed to alter the ancient ſyſtem of fermentation, I ſhould be a good deal inclined to add a new ſtage to this proceſs, and ſay, that fermentation conſiſts of four ſtages, the ſaccharine, the vinous, the acetous, and putrefactive. But it is rather too ſoon to propoſe this alteration, though what has been mentioned may ſerve to ſhew that certain mixtures of farinaceous matter with ſweet matter may be wholly changed into ſaccharine matter, and the ſweet matter with which it is mixed might be conſidered in the ſame light with yeſt in the vinous fermentation. But many things are wanted before this opinion can be ſupported by experiment.

When the ſweet matter mixed with farina is ſmall in proportion to it, the whole falls eaſily into the ſtate of vinegar. Hence, the ſame principle, which in favourable ſeaſons, when the ſweet matter is in great quantity in the grape, contributes to the ſtrength of

the vine, in a bad ſeaſon, promotes its acidity. It may, probably, alſo happen, that from want of proper maturity, the mucilage in the grape may ſometimes be in too large proportion, the quantity of ſaccharine matter being no greater than its uſual quantity; but this is mere conjecture, and takes place very rarely. When this is not the caſe, when the only fault in the grape is, its being too watery, from too much rain in the end of the ſeaſon, this may be cured in part, by taking the juice after it is ſqueezed, and depriving it of a little of its water by expoſing it for ſome time to heat.

The ſweet matter thus concentrated, may be mixed with ſome juice in its natural ſtate: the whole thus becomes fitter for producing a ſtrong wine. This operation is often performed, even when the vintage is favourable, and the juice of the grape ſufficiently ſweet. By this means, a ſtronger and ſweeter wine is produced than the juice of the grape in its ordinary ſtate could have yielded. The part of the juice which was deprived of ſome of its water by heat, ferments with greater difficulty, the wine retains a ſweet taſte for a longer time from this cauſe, and this taſte, by very ſimple operations may be retained for a very long time.

The concentration of the juice of the grape by fire is chiefly applicable to the preparation of ſweet wines; thoſe that are valued for their fine flavour and ſharpneſs cannot be prepared in this way in any tolerable degree of perfection. The ſweet matter

in the grape being in greater or ſmaller quantity, in proportion to the other parts, and varying in quality as well as quantity in different ſoils and ſeaſons, and having its properties changed by fire, occaſion great differences in the wine. It may alſo be added, upon this ſubject, that even in moſt favourable years, it never happens that all the grapes in a vineyard, or even all the berries in one cluſter of grapes acquire the ſame degree of maturity at the ſame inſtant. Great attention is neceſſary to ſeparate thoſe that are not perfectly ripe, and thoſe that are any way injured from the reſt. The more accurately this is performed, the finer will the wine be, and from this circumſtance, great varieties are produced, and the wines from vineyards in the ſame ſoil, and otherwiſe managed in the ſame manner, differ as much from one another, as the wines of different ſeaſons and ſoils.

Beſides theſe cauſes of differences in ſubſtances that have undergone the vinous fermentation, another may be mentioned, the effect of ſoil and climate upon the odorous principle of the grape. In this, as well as moſt fruits, which have been much cultivated, there is a vaſt variety produced by culture. Though all of theſe are compoſed of nearly the ſame principles, ſmall differences are perceived. The ſmell and flavour differ remarkably in different individuals. The liquor, after fermentation, retains in a great meaſure the original flavour of the fruit. This is the chief cauſe of the delicate differences

among wines. Proprietors of large vineyards have an opportunity of producing more high flavoured wines than the proprietor of a ſmall one. Their wealth and connections give them opportunities of collecting every ſpecies of vine which has been eſteemed for its flavour; they even try to obtain new varieties. At the ſame time, by mixing grapes differing in flavour, a wine is often produced ſuperior to what any ſingle ſpecies could have yielded.

Beſides the cauſes already mentioned of the differences in bodies which have paſſed through the vinous fermentation, ſeveral others might be enumerated. I ſhall only mention two. The firſt is, the cauſe of the different colours in wine: the other the cauſe of their harſhneſs, their ſweetneſs, their ſparkling, or not ſparkling in the glaſs. There are two ſpecies of grape, of which the colour only is attended to, the white and the red grape. Originally, perhaps, white wines were made from white grapes only, and red from red grapes. At preſent red wine is ſometimes made from white grapes, but all white wines that are valued, as well as all the admired red wines, are the produce of the red grape. The juice of the red grape is as colourleſs as that of the white. The colour reſides in the ſkin. It is eaſy to prevent the colouring matter of the huſk from affecting the juice, by preſſing the grapes gently, and taking care to do this without their having been expoſed to the ſun after being pulled and picked. If the juice is wanted of a red colour, the ſun is

allowed to beat upon the pulled grapes before they are preſſed. Some of the ſkins are allowed to remain mixed with the juice after it is ſqueezed out, and ſometimes, though rarely, the ſkins are boiled in water, and the water mixed with the juice, or the ſkins are boiled with a part of the juice, and then mixed with the reſt. According to the proportion of colouring matter that has mixed with the juice, the wine is either almoſt as colourleſs as water, or of a deep red, and all the intermediate ſhades may in this manner be produced from the red grape. White and red Champaigne are made in this manner from the ſame grape, and the ſtill more admired kind, the yeux de Perdrix. When red wine is to be made from the juice of white grapes, ſome colouring drug, as turnſol or archil is neceſſary to die the juice of the deſired colour: but this is a bad practice, is now laid aſide almoſt entirely, and can hardly be ever uſeful, as the red is as hardy as the white grape. The ſweetneſs or harſhneſs of wines, though it ſometimes depends upon the watery parts of the juice having been forced away by fire, is ſometimes produced by checking the fermentation before it is completely finiſhed, and putting the whole into caſks, in which one half of an inch of a brimſtone match has been burned. The fermentation ſtops, and the liquor retains all the ſweetneſs it poſſeſſed, when this operation was performed. Such wines ar called matched wines, but are not ſo much eſteemed as thoſe whoſe ſweet taſte is owing

to the cauſe mentioned above. By ſtopping the fermentation before it naturally ceaſes, various differences in wine are produced. I need only mention that the moſt remarkable of theſe, the ſparkling quality, is owing to the fermentation being checked, and the liquor put into the bottle, and the fermentation being completed without the gas, which is extricated, being allowed to eſcape, which of conſequence is obliged to combine with the watery part of the wine. Vinous fermentation produces ſimilar differences upon the ſweet matter of other fruits as upon the grapes, and ſimilar differences are obſervable in the liquor produced by fermenting many other ſpecies of ſweet matter, as ſugar, honey, molaſſes, and malt.

Though, however, theſe differences do actually take place, yet they are not ſo much attended to, as the difference ariſing in the grape, from the cauſes already mentioned. Wine is made principally for the purpoſe of being drank without any farther preparation. The ſweet matter of ſugar is fermented with a view to the ſpirit, which will be furniſhed by a ſubſequent diſtillation. The ſweet matter of malt is fermented more on account of the uſes to which it is applicable in the ſtate of ale or beer, than on account of the ſpirit which it yields by diſtillation, and therefore furniſhes a farther proof of the application of the preceding principles. Even, however, in the inſtance of ſugar or honey, they may be perceived. When the juice of the ſugar-cane is fer-

mented in the ſtate it is in, as obtained by preſſure from the plant, a more compounded ſweet matter is obtained than pure ſugar and water. For beſides theſe a little oily or fragrant matter is obtained, mixed with viſcid farinaceous or extractive parts. The fermented liquor partakes of the original qualities of thoſe ſubſtances, and ſome of them accompany the ſpirituous part ſeparated by diſtillation. Hence this ſpirit is of a more fragrant kind than that made of the refuſe of the ſugar boiler. When the juice of the ſugar cane has been boiled, and ſugar ſeparated from it, the refuſe, the molaſſes and ſcummings, furniſh a ſpirit leſs fragrant.

But no differences can be perceived in this way, excepting ſuch as ariſe from ſubſtances that are volatile, the reſt remain in the liquor after the ſpirit is ſeparated, and paſs unnoticed or diſregarded. In ſome caſes, however, fragrant ſubſtances are mixed with ſugar and water, in order to furniſh a liquor more agreeable than pure ſugar would produce, and therefore to be uſed in the vinous ſtate upon the principle, that the fragrant matter will remain after the fermentation. All kinds of made wines belong to this head, and though in this country they are looked upon with contempt, yet they are as wholeſome, and moſt of them as palatable, as thoſe produced from the grape. Of all the ſweet ſubſtances fermented for the ſake of the ſpirit, the one that contains the greateſt mixture, is that from which arrack is made—the juice of the palm tree, and the

milk of cocoa nuts. The firſt of theſe is compounded like the juices of other ſweet plants; the latter contains, as I have found by analyſis, a quantity of ſweet matter mixed with oil and farina, and ſomething of a narcotic ſubſtance, but the laſt is in very ſmall quantity, though ſufficient to make the milk, when freely drank, produce dangerous, and almoſt fatal effects. Were we to examine the juice of the palm and the milk of cocoa, immediately after fermentation, we ſhould find a very compounded and diſagreeable liquor; by diſtillation theſe are partly ſeparated; much oily odoriferous matter diſtils from the ſpirit, and ſerves to diſtinguiſh it from all other fluids. Malt is chiefly uſed for the ſake of the vinous liquor which it yields by fermentation.

ESSAY IX.

ON FERMENTATION.

In a former eſſay I conſidered fermentation in general, and the various means of promoting or retarding it. I mentioned the three ſtages into which it has been generally divided; the vinous, the acetous, and putrefactive. I took notice of the general diſpoſition in bodies to fall into theſe ſtages; that ſweet ſubſtances are capable of being changed in the greateſt variety of ways by fermentation, and therefore that the nature of fermentation can moſt eaſily be diſcovered by an attentive obſervation of the ſweet ſubſtances in the various ſtages of this proceſs; that the ſweet matter in fruits was probably firſt fermented, and is of the ſimpleſt kind. This I illuſtrated from the nature of the proceſs for making wine, and pointed out the cauſes of the differences

among wines. They all conſiſt of the ſame ſubſtance fermented in nearly the ſame way. I ſaid that the nature of fermentation is but little underſtood, in ſo far as the cauſes of the changes produced by it are concerned; that it was not my intention to give any theory of fermentation, but to give an account of the phenomena, to prepare us for the theory in due time.

The effects produced by fermentation are more eaſily obſerved when the liquor fermented is uſed immediately, or at leaſt without any further operation. On this account I ſhall not mention at preſent the changes produced by fermentation upon ſugar, and other purer ſweets, but confine myſelf to the fermentation beſt known in this country, and endeavour to mention ſome of the cauſes of the differences produced by fermentation upon the ſweet matter in malt.

The fermentation of grain furniſhes greater variety than that of any other ſubſtance. This is owing partly to the farina it contains, and to the changes which this farina may be made to undergo by vegetation. The ſeeds of moſt of the culmiferous plants are nearly of the ſame nature, and all of them may be fermented into a vinous liquor. They all contain, beſides the rudiments of the young vegetable, a quantity of a ſubſtance to nouriſh and ſupport it, till it acquires ſufficient ſtrength to prepare food for itſelf. The part chiefly employed is

farina, ſometimes mixed with more, ſometimes with leſs of a ſweet matter. Farina is an inſipid inodorous ſubſtance, not ſoluble in cold water, and forming with hot water a glutinous maſs. It is never found quite pure in the ſeeds of plants. It is mixed not only with vaſcular parts of the young vegetable, but united ſometimes with a gluten, ſometimes with ſweet, ſometimes with oily, or bitter, and narcotic matter. The farina is extremely nouriſhing to animals, and it is not improbable that the nouriſhing quality of all vegetables depends on the portion of it which they contain.

Thoſe ſeeds which contain leaſt foreign matter mixed with farina, are preferred to the reſt, as they yield not only the moſt agreeable food, but are likewiſe moſt capable of being applied to uſeful purpoſes after fermentation. Hence wheat, barley, and oats, and ſome others, are preferred to the reſt. When the farinaceous part is quite pure, it is hardly capable of being fermented at all, but is the moſt incorruptible of all vegetable ſubſtances. When mixed with ſweet matter, it may be fermented in many different ways, according to the manner of managing it. The farina in the ſeeds of plants is always mixed with ſweet matter, and may be uſed for the purpoſes of life, in one or other of theſe two ſtates. Theſe ſeeds may either be fermented in their natural ſtate mixed with a little water, or they may be fermented after they have begun to vegetate, when mixed with much water. In the firſt caſe

they are uſed as food, and do not acquire the properties of wine; in the latter, they fall into the vinous fermentation, and loſe the greateſt part of their nutritive quality. The farinaceous ſeeds ground into powder and expoſed to the circumſtances favourable for fermentation very readily become ſour, or acquire the properties of leaven. If mixed previouſly with a quantity of any ferment from a vinous liquor ſuch as yeſt, a real vinous fermentation takes place, the viſcidity of the farinaceous matter is diminiſhed, and the whole becomes more ſpungy; water penetrates it more eaſily than before, and it mixes more readily with any watery fluid which is taken along with the food. During the fermentation a quantity of aërial matter ſimilar to that from fermenting wine ſeparates. The maſs does not acquire any intoxicating quality, from the farina being in a ſtate different from the ſweet matter, and the quantity of the latter being too ſmall to produce a complete change upon the whole. There, however, can be hardly a doubt that a farinaceous ſeed, when fermenting in the manner juſt related, would really yield ſome ardent ſpirit: though no attempts that I know of have been made to collect it. The fermentation is never allowed to proceed far, leſt the maſs ſhould become ſour, and the whole is expoſed very ſuddenly to a conſiderable degree of heat, to ſtop the fermentation completely as ſoon as poſſible.

When the ſeed contained originally but little ſweet matter the fermentation ſeems to be leſs of the vinous

and more of the acid kind, and in many inſtances ſeems to be of the acetous nature altogether. The farina in the ſeeds of vegetables is changed by vegetation into a ſweet ſubſtance eaſily miſcible with water. By this means they are converted into malt. But the change of the farina into ſweet matter by vegetation is not in proportion to the original ſweet matter mixed with it. Wheat and barley are inſtances of this: the firſt contains moſt ſweet matter in its natural ſtate; the other having vegetated. Vegetation ſeems in this inſtance to change the nature of barley more eaſily into ſweet matter than it does the farina of wheat, and is capable of producing more ſweet matter from the one than it can from an equal quantity in the other. The cauſe of this is not eaſily aſſigned; the fact, I believe, is indiſputable, and the ſolution of the difficulty will be found to depend upon the mixture of a ſubſtance with the ſweet and farinaceous matter in wheat, and which, when ſeparated, is of a glutinous quality, and will neither diſſolve in hot nor cold water, and has ſome of the properties of animal ſubſtances.

Vegetation not only affects the farinaceous matter differently in different ſeeds, but produces different effects upon the ſame ſeed, by which the nature of the farina is more or leſs altered according to the different degrees of perfection to which the ſeed has arrived. Much depends upon its degree of maturity. When the ſoil and ſeaſon are good, the quantity of ſweet matter produced by vegetation is greater and purer,

and it is produced in leſs time. Hence it is found neceſſary to keep the grain of different ſoils ſeparate. Were they expoſed to the malting proceſs together, it would be over in one kind, before it was well begun in another, and if continued till it was complete in the latter, the former would be ſpoiled. The ſame attention is neceſſary in grain of different years. The longer the grain has been kept, the more time is neceſſary to change the nature of the farina contained in it.

Even when the grain is all preciſely of the ſame quality, vegetation in different degrees or differently conducted, will alter the quantity and quality of the ſweet matter. Many ſtill more minute differences might be mentioned, which would be found uſeful in accounting for the ſmaller differences produced afterwards by fermentation. The perfection of the ſweet matter in grain depends upon bringing the ſeed to a preciſe point of vegetation. If it is carried beyond this point, the ſweet matter is changed into a ſtate unfavourable to the production of a perfect vinous liquor; if it is ſhort of it, a part of the farina remains in its natural ſtate, which, if not unfavourable to fermentation, at leaſt diminiſhes the ſpirit in it. In ſome inſtances, however, the vegetation is ſtopped before the whole farina is changed into ſweet matter, in order to obtain afterwards by fermentation a milder and more viſcid, though leſs ſpirituous liquor. When this is intended, the grain is juſt allowed to begin to grow, and the moment the radicle makes

its appearance, the vegetation is ſtopped. When a thinner, lighter, harder, and ſtronger liquor is wanted, the vegetation is allowed to proceed farther, and is not completely ſtopped till the plume has become as long as the ſeed. In this caſe, the ſweet matter in the grain is in greateſt quantity, and has acquired the properties beſt fitted for undergoing completely the vinous fermentation. In all caſes, however, the ſweet matter ought to be in the ſame ſtate in the whole grain, upon which the malting proceſs is performing. To render therefore, the vegetation equal in every grain of which the heap is compoſed, is the great object in view, and this is attempted by frequently turning it, that every grain may be equally expoſed to the air, and the whole brought to the deſired point if poſſible. When this is accompliſhed, the whole ſhould be withered, by ſpreading it thin on the floor and drying it as ſoon as poſſible. There is one circumſtance worthy of notice in this proceſs, which has not been mentioned by any writer, and which I imagine, is not ſo much attended to as it ought to be, even by thoſe whoſe profeſſion it more particularly regards, I mean the quantity and equal diſtribution of the light in the apartment where the proceſs of malting is carried on. It is known to every body that light is of the utmoſt importance in vegetation, that vegetables will not grow in a dark place, at leaſt that they do not arrive to any degree of perfection in this ſituation, and never contain any thing but an inſipid

watery juice; and that all plants growing in a dark place have juices of the ſame qualities. If a ray of light is admitted into the dark apartment, the vegetable grows with greater vigour, and directs itſelf to the aperture by which the light entered. Even different degrees of light produce ſenſible differences; vegetables growing in a ſituation moſt expoſed to the ſun have the richeſt juices, and this is in no caſe more remarkable than in the production of ſweet matter, as ſeems to be evident from the ſweet matter in grapes on the ſunny ſide of a hill, being in greater quantity than in grapes of the ſame ſpecies gathered from the north ſide of it. One might expect that the ſweet matter in grain beginning to vegetate ſhould alſo be affected by light. The ſeeds of plants growing below ground may indeed be ſuppoſed to be little affected by light: but grain in malting muſt be affected by it. Even the ſeed below ground is not altogether out of the reach of the influence of light, as is evident from comparing the growth of ſeeds in different pots of earth, ſome of which are expoſed to the light, and others kept in a dark place, the heat in all being equal. It is a common obſervation, that the ſweet matter in malt and the other liquors made from it, is ſubject to great variation. No doubt this is in part owing to the differences betwixt different ears of barley in the ſame field, or to differences ariſing from moiſture, or the different degrees of expoſure to the air. But it ſeems reaſonable to ſuppoſe that part of thoſe

differences ariſe from the different degrees of light to which the grain has been expoſed during its vegetation. In moſt caſes the light admitted is not great, but ſtill uniformity in this reſpect would probably be of advantage. I ſuppoſe, one reaſon why much light is not admitted in the malting proceſs, is that the vegetation would proceed too rapidly and be more difficult to ſtop at the proper point. I ſhould wiſh likewiſe the experiment were tried to aſcertain what would be the effect of a perfectly dark place upon grain beginning to vegetate, in ſo far as the nature and properties of the ſweet matter contained in it were concerned.

Beſides the differences produced by fermentation upon grain in its natural ſtate, and after being malted, other differences ariſe from the operations it is expoſed to, or the additions made to it before fermentation. Of the differences ariſing from alterations produced by operations previous to the fermentation, the moſt remarkable are produced by heat. This muſt always be employed after the grain has vegetated, in order to put a ſtop to the vegetation at that point when the ſweet matter is in greateſt quantity, or beſt ſuited to the kind of liquor to be made from it by fermentation. If the water which the grain abſorbed in the operation previous to its malting, be allowed to remain after that proceſs is over, the ſweet matter will ſoon be diminiſhed, and acquire a ſtrong tendency to the acetous fermentation. In all caſes therefore the grain muſt be dried to preſerve it in its moſt perfect ſtate for

the purpoſe intended. There are two ways of doing this; one by expoſing the grain to an exceſſively hot ſun, which ſoon dries up the moiſture without producing any material alteration upon the other parts. In this manner the colour remains unaltered; and when water is afterwards employed, a very pale infuſion is obtained, and after fermentation the liquor differs but little in colour from water. When the vegetation is checked by heat, raiſed by fuel, the ſweet matter is always more or leſs altered, and the ſcarf ſkin of the grain becomes of a darker colour, and the whole communicates to water a deep yellow or brown colour. Something likewiſe depends upon the kind of fuel uſed. If the fuel employed has any ſmoke, a part of it condenſes upon the grain, and communicates a taſte to the infuſion and to the liquor after fermentation. The greater the degree of heat employed, the darker coloured is the infuſion. It is, however, poſſible to employ fuel, if it has no ſmoke, in ſuch a manner as not to affect the colour of the infuſion. By throwing a little ſulphur upon the fuel the malt will give a paler colour to water. If ſulphureous coals are employed the ſame effect will be produced; but the fermentation of the infuſion will proceed in a very different manner from the effect which the acid of the ſulphur communicates to the ſweet matter. This practice ſeems to be a dangerous one, and can hardly be executed with any proſpect of ſucceſs.

Beſides the effects already mentioned, the greater the degree of heat to which the grain has been

expoſed, the more difficult will it be to bring on the proceſs of fermentation: and it may happen from this cauſe that it will become altogether impracticable. To this head alſo belong the effects depending upon the heat of the water by which the ſweet matter is extracted. One circumſtance muſt particularly be attended to, that the heat be not ſo great as to reduce the malt into a pulpy ſtate, from which the water will not run. This will certainly happen if the water be poured boiling to the malt ground into powder, and even when bruiſed, which is done to render this leſs liable to take place. Beſides the difference in the heat of the water, the time it is allowed to remain upon the malt, and the precautions uſed to preſerve the heat, occaſion ſeveral alterations in the liquor produced by fermentation. The more agreeable parts are extracted firſt; if the water is drawn off as ſoon as the heavy parts are depoſited, a more agreeable liquor will be obtained, and the ſweet matter remaining may be extracted by pouring more water and employing this ſecond infuſion to inferior purpoſes. The great delicacy of the buſineſs ſeems to depend upon the heat of the water. No part of the operation is leſs generally known, and in none has the great manufacturer ſo much the advantage over the ſmall one. When great quantities of materials are uſed at a time, they preſerve their heat longer, and by this means a more moderate heat is capable of producing a greater effect. The thermometer has been called to aſſiſt in this part of the

operation, inſtead of uſing the hand of the brewer as a meaſurer of heat. The degree of heat moſt favourable for extracting the ſweet matter, without making it into a pulp and without injuring the ſweet matter or the ſubſequent fermentation, ſeems to be the great ſecret, and is anxiouſly concealed by the people in the trade. They even conceal it from the thermometer makers, by ſubſtituting letters for numbers to mark the degrees in their thermometers. So far, however, as I have been able to learn they uſe different degrees of heat for different purpoſes, and that from 163 to 168 of Fahrenheit, is moſt generally employed in making the finer ſpecies of malt liquors. It happens too, very frequently, that differences in the liquor after fermentation ariſe from the time the infuſion has been boiled before this proceſs is begun. This is always neceſſary to prevent, it is ſaid, the danger of ſouring, and alſo to condenſe the infuſion ſo as to furniſh a ſtronger liquor. It is alſo employed to mix ſubſtances with the infuſion to preſerve the liquor after it is fermented; how far it is neceſſary on the firſt of theſe accounts is not known; perhaps the ſubſtances mixed commonly with water may render it neceſſary. Something may depend on ſubſtances contained in the water. Soft water is ſuppoſed in general to be preferable to hard, and the water of certain rivers, like the Thames, ſuperior to all others.

The differences produced by addition previous to the fermentation deſerve alſo to be mentioned. Of theſe the moſt common and moſt uſeful is the

addition of bitter plants to the infuſion of malt. The plant now moſt generally preferred is the hop, a ſubſtance of an intenſe bitter, mixed with a little aromatic matter. This is added with a view chiefly to preſerve the liquor from becoming ſour after it is fermented, to which every malt liquor is very ſubject. With this intention the hop is boiled in the wort. This ſeems, however, to be ill calculated for the purpoſe. By this means the aromatic part is loſt, which might eaſily be retained by ſteeping the hop in a cloſe veſſel in ſome hot water, and afterwards mixing this with the reſt when removed from the fire. Some may perhaps imagine this to be injurious: but it has been tried by many at my requeſt with ſucceſs. The bitter taſte communicated by this vegetable gradually diminiſhes, and at laſt ceaſes or dies. This is owing to the gradual formation of an acid in the fermented liquor, and any bitter ſubſtance may be inſtantaneouſly deprived of its taſte by adding a ſufficient quantity of acid to it. This I have long employed as a teſt of the quantity of bitter matter in bodies.

Sometimes common ſalt is added to the water intended to be uſed in extracting the ſweet matter of malt. This addition, it may be ſuppoſed, is made with a view chiefly to excite thirſt, but beſides this effect it produces alſo ſome others, particularly it moderates the fermentation, makes the liquor fine, and ſeems to be the cauſe of the great ſuperiority of the water of certain places in producing fine malt liquor. The water of the Thames

is remarkably celebrated on this account, and it contains a little ſalt; and in moſt places remarkable for fine malt liquor, I have found this alſo to be the caſe. To anſwer theſe purpoſes the quantity of ſalt contained in the water ſhould be ſmall; no advantage will be obtained if the quantity is ſo great as to be diſcovered by the taſte.

Beſides theſe cauſes of differences in malt liquors there is one leſs generally attended to, ariſing from the different nature of the grain in different ſoils and ſeaſons which occaſions varieties in the liquor produced, though the proportion of grain and water be the ſame. This principally affects the ſtrength of the liquor, and it were to be wiſhed ſome accurate means could be contrived to make the water and ſweet always bear an accurate proportion to each other. It has accordingly been lately propoſed to accompliſh this by means of an inſtrument which will ſhew the quantity of matter extracted by water from the grain without its being neceſſary to know what quantity of each had been employed. The inſtrument is conſtructed like the common hydrometer, is made of glaſs or metal, and points out by the quantity of the inſtrument which ſinks, how much the fluid differs from pure water. The leſs it ſinks, the more has the water diſſolved, and the ſtronger it is of the ſoluble matter of the body on which it was poured. This inſtrument, when put into an infuſion of malt, ſhews how much has been diſſolved, and as the ſoluble part in malt is of a

ſaccharine nature, the inſtrument has been called a ſaccharometer. It is, however, difficult to make a ſcale to it, ſo that obſervations made by different perſons may be compared, and it is liable to ſome variations from the heat of the fluid in which it is immerſed varying, as well as from differences in the matter diſſolved from the cauſes already mentioned. With ſome attention theſe might probably be leſſened or removed, in which caſe it would prove of conſiderable utility. Some of the differences in fermenting malt liquors alſo depend upon the manner in which the fermentation has been conducted. The ſweet matter in malt does not fall readily of itſelf into the vinous fermentation. Even when expoſed to the moſt proper degree of heat, in the moſt favorable circumſtances, it is more diſpoſed to become ſour.

The various heterogeneous ſubſtances mixed with the ſweet matter in malt, together with the heat employed in drying it, probably occaſion this tendency and afford a reaſon why little artificial heat is neceſſary for the perfect fermentation of liquor made from malt. This is ſo much the caſe that in warm climates and ſeaſons it is hardly poſſible to produce tolerable malt liquor; and to the ſame circumſtances it is not improbable we owe the ſuperiority of that made in October and March, and diſtinguiſhed by the names of the months in which it was made. In the moſt favourable ſeaſons and circumſtances ſome fermenting ſubſtance muſt be added to diſpoſe

the whole to the vinous ſtate. And this generally conſiſts of the ſcum which riſes from liquors in the ſtate of vinous fermentation. How this ſubſtance acts is not eaſily explained, but ſome of its properties are known. It ſeems to be of a farinaceous nature, and is eaſily preſerved with all its properties in a moiſt ſtate for many months, or ſmall twigs dipped in it and dried will retain the property of exciting the vinous fermentation for a long time. It has indeed been aſſerted that yeſt is nothing more than a ſubſtance ſtrongly impregnated with fixed air. But this can ſcarcely be imagined to be the caſe when it is preſerved in a dry ſtate. Though the vinous fermentation is produced with difficulty in infuſions of malt, yet if once raiſed, it proceeds with great violence and is very apt to proceed beyond the proper boundary. The fermentation is on this account always checked before it is completely finiſhed, and the remainder of the proceſs is performed in a very gradual manner, and often takes years in being completed. To know when it is proper to check the fermentation is a circumſtance of great importance. No other mark has been commonly uſed excepting the change in taſte of the liquor. As this is ſubject to great irregularities, it has been propoſed to obtain a more perfect meaſure of the progreſs of fermentation from the change produced in the denſity of the fluid. The ſame inſtrument which has been propoſed to meaſure the quantity of the ſweet matter mixed with the water before

the fermentation, may alſo be uſed to meaſure the alteration in denſity produced by fermentation. When uſed for this purpoſe it ſeems to be liable to fewer objections. It will no doubt require nice obſervations to find out accurately the change in denſity which ſhould be produced in every caſe according as the infuſion was ſtrong or weak, according as it is intended that the liquor ſhould retain ſome of its ſweetneſs after fermentation or loſe it intirely, and according as it is intended that the liquor ſhould be uſed ſoon after the fermentation or kept for a conſiderable time after it. At whatever period it may be judged neceſſary to ſtop the fermentation, it will always be neceſſary to put the liquor into clean veſſels, and to keep the whole for ſome time at leaſt in a cool place and in cloſe veſſels. After all the operations to which the malt liquor has been ſubjected, we ſhall find it more compounded than wine, and that it will retain much viſcid matter, which was originally mixed with its ſweet matter, together with ſome vinegar which has been formed from the action of the viſcid parts on the ſweet matter during the vinous fermentation, by which means the liquor, though more nouriſhing than wine, is not ſo light on the ſtomach, is more heating, and is beſt fitted for the conſtitution of laborious people.

ESSAY X.

ON CEMENTS.

Read A. D. 1782.

CEMENT in building was probably uſed ſoon after human habitations began to be built of ſtone. Without ſome invention of this kind it is evident, that houſes could afford but a very imperfect defence againſt the weather, and though they might be tolerable in a very warm climate, they would furniſh an uncomfortable lodging in a cold one. What ſubſtances were firſt employed to join ſtones together in building, it is, perhaps, at preſent impoſſible to determine with preciſion. It ſeems, however, reaſonable to ſuppoſe that the ſubſtances firſt uſed for this purpoſe were poſſeſſed of the more obvious properties of cements, to wit, tenacity and ductility when wet, with a conſiderable hardneſs when dry. The body which poſſeſſes theſe qualities moſt remarkably in a ſtate of nature is clay. When mixed

with a little water, it is extremely tenacious and ductile, capable of being ſpread to almoſt any degree of thinneſs, and when the water is ſeparated from it by the ſpontaneous heat of the weather, it becomes hard, and makes the ſubſtances between which it was ſpread cohere with a ſmall force. As the clay however, is liable to be penetrated by water in very wet weather, it will be apt firſt to be reduced to its original ſoft ſtate, and then to be waſhed away by the rain. For both theſe reaſons buildings in which clay is the cement can have but little durability. Beſides the inconveniences juſt mentioned, clay, when mixed with water, is apt to crack in drying, by which means a great number of crevices will be formed in the building in which it is uſed as a cement; through theſe the wind and rain will freely paſs, ſo as to render the houſe extremely uncomfortable during winds; and in wet weather, the rain being more freely admitted, will occaſion damp, and effectually contribute to the deſtruction of the edifice, the cement itſelf being quickly waſhed away.

The inconveniences of clay as a cement, from the cracks which are formed in it on drying, might be remedied by mixing ſuch ſubſtances with it as are leaſt penetrable by water, and which of conſequence are leſs apt to have their dimenſions altered by drying. Sand is of this kind; but a mixture of clay and ſand has leſs tenacity than clay, and is more eaſily penetrated by water; though as it does not crack in drying, it is ſtill frequently employed in coarſe

buildings. Of this kind alſo are moſt dry vegetables, particularly ſuch as having arrived at perfect maturity by completely ripening their parts, have little water in their compoſition. Annual plants, in which the vegetation ceaſes in the roots as ſoon as the ſeed is ripe, have leſs water in them than other plants in the ſame ſituation, and are therefore leſs liable to alter their dimenſions by being expoſed to a dry air. Theſe ſubſtances mixed with clay would remedy in part the inconveniences ariſing in drying. Straw or ſtubble poſſeſſes the properties alluded to in the greateſt degree, and would probably be uſed in mixture with clay when intended to be employed as a cement. If theſe could not be had, the withered leaves of trees might be ſubſtituted for them. Such mixtures of clay and dry vegetables have been uſed as cement in building among many nations, and are ſtill uſed by the more uncultivated parts of our own. The buildings thus conſtructed are, however, neither elegant nor durable. And though the addition of vegetable matter may prevent the cracking of the clay by the ſeparation of the water, yet that very matter is ſubject to decay in a certain time, and, by the gradual diſſipation of parts produced by putrefaction, to loſe all the properties for which it was originally uſed. By theſe means the cement in a certain time becomes more porous, and more eaſily penetrated by the wind and rain, as well as more liable to ſcale off, than if no ſuch addition had been made.

The inconveniences of this cement muſt ſoon either have made mankind lay aſide ſtone in building, and have recourſe to their old material of wood, or try by various experiments to diſcover a cement more perfect than that made of clay. It is ſuppoſed by ſome, that fire was uſed to make the ſtones in buildings cohere, that houſes in ſome places were made of a very fuſible kind of ſtone, that afterwards a great fire was kindled in the houſe ſufficient to melt, at leaſt to ſoften the ſtones of which it was made, by which means the whole ſtones cohered and became as one piece equally impenetrable to air and water. Such, it is ſaid, were ſome buildings in Scotland, which have been deſcribed under the name of vitrified forts. Concerning theſe it may be obſerved that, granting the fact, this, though a very durable, would be a very expenſive mode of building. It would be uncertain in many caſes, apt to fail either by too ſmall or too great a degree of heat; too little making the joining imperfect, and too much diſtorting the figure of the building. Nothing elegant could be produced by this mode of operating, and the proceſs would fail altogether excepting when the ſtone was of a very fuſible kind. A houſe of marble treated in this manner would crumble into powder. The joining of freeſtone and granite would be as imperfect after this operation as before it, and the only ſpecies of ſtone in which it could at all ſucceed is whinſtone, and ſome others like it. Perhaps very bad bricks might be joined in this

manner. If the walls were of any great thickneſs the inſide would be in the ſtate of glaſs before the outſide had acquired a red heat. This method of cementing ſtones in building muſt be ſubject to numerous inconveniences even in the moſt favorable ſituations, and in many would fail altogether.

Buildings of this kind are, I believe, ſeldom to be met with but in high grounds, or on the tops of hills, where the wind, blowing with more violence, might raiſe the fire to a greater degree than could eaſily be done in a lower ſituation. In this reſpect the vitrified forts reſemble the furnaces which our anceſtors conſtructed on the tops of hills for the purpoſe of ſmelting metals from their ores, before the mechanical arts had arrived at ſuch a ſtate of improvement as to admit their being conſtructed in leſs elevated and more convenient ſituations.

As the practice of cementing ſtones by means of fire ſeems never to have been very general, other means have been fallen upon by which the ſtones in buildings might be more conveniently and as effectually joined. The ſubſtance which has been moſt generally employed for this purpoſe is the calcareous earth in the ſtate of lime mixed with certain ſubſtances, and reduced to a proper conſiſtence by means of water. This mixture, when properly made, produces the beſt cement hitherto uſed in building. But its qualities differ very much according to the preparation which the calcareous earth has undergone, and the ſubſtances that have been mixed with

it. The calcareous earth in its crude ſtate is of no uſe in building. When reduced to powder and mixed with water, it has no tenacity, and upon the ſeparation of that water is equally dry and void of coheſion. When the calcareous earth has been expoſed to the utmoſt violence of fire for a long time, it becomes very brittle, loſes conſiderably of its weight, becomes very hot on throwing water on it, and falls into a bulky powder, which is, however ſtill very dry. Upon mixing more water with it, it forms a paſte poſſeſſed of a conſiderable tenacity, and this by long expoſure to the air becomes gritty in its particles, but does not cohere ſo as to form a ſolid maſs. It conſiſts of a number of hard particles, but has no properties that would make one uſe it as a cement in building. When a little lime is diffuſed with a great deal of water, it diſſolves and forms lime water; and by expoſing this water to the air in a glaſs, there forms a very hard cruſt on the veſſel which ſeparates from it with difficulty, and ſeems to have all the hardneſs which the calcareous earth can acquire. If one could give the whole maſs the tenacity and hardneſs which the little contained in lime-water is capable of acquiring, it would poſſeſs all the qualities of a good cement.

When we examine into the cauſes of this difference in the ſame ſubſtance expoſed to air in theſe two ways, we ſhall find that the want of coheſion in the large maſs is principally owing to the altera-

tion in ſize by the ſeparation of water, and attraction of part of the air in conſequence of expoſure. If one could remedy the inconveniences ariſing from this ſource, a perfect cement would be produced, provided the calcareous earth was originally ſufficiently pure, and the degree of heat to which it was expoſed had been ſufficiently violent. A very ſmall quantity of lime is ſufficient to give tenacity to a great quantity of other materials. It has been found that lime requires ſome additions to fit it for a cement, and theſe are to prevent cracking by the ſeparation of water, which all cement muſt contain, and to enable the lime to abſorb air, and to have that chemical effect produced on it, by which it becomes hard and capable of making the ſtones cohere. The cracking of the cement ariſing from the alteration in its bulk by the ſeparation of water may be prevented in ſeveral ways. The moſt common is to add hair if the cement is to be applied to the inſide of houſes, and ſand or ſiliceous earth in the body of the houſe and where it is expoſed to weather. Sand, in the compoſition of cement, anſwers ſome other purpoſes beſides preventing cracking from the diſſipation of the water. Sand is perfectly inſoluble in water, and is incapable of being acted upon by the lime, it does not alter its dimenſions at all when made an ingredient in cement, and therefore occaſions a leſs alteration than would have taken place had the whole maſs conſiſted of lime. That ſand may anſwer this purpoſe it is neceſſary it ſhould be pure, and par-

ticularly that it ſhould be perfectly free of clay or mud, both of which imbibing water and altering their dimenſions by it are ſubject to the ſame inconvenience that lime itſelf is, and to remedy which ſand was added. River ſand, which has leſs mud or clay than pit ſand, is univerſally preferred as an ingredient in cements for building.

A proper mixture of ſand and ſlaked lime in proceſs of time becomes hard without cracking, the hardneſs increaſes with age, and it is impervious to water. The hardening of the cement in building is entirely owing to the effect of the air on the lime, and not to the ſeparation of water from it. By recovering that air which the fire had ſeparated, it recovers, in a great meaſure, the hardneſs which it had before burning. The ſooner lime can recover this air, the quicker will the cement harden; and the more equally the air is furniſhed to the lime, the more uniform will the maſs be, and the greater the coheſion of the whole. That the air may be eaſily and equally furniſhed to the lime, it is neceſſary that the lime ſhould be equally and perfectly calcined, and the cement made of it be as equally expoſed to the action of the air as poſſible. This is effected firſt by uſing the lime as ſoon after it is burnt as poſſible, and ſecondly by mixing ſand or other ſubſtances with it in ſufficient quantity and of a proper ſize. That the uſing lime new-ſlaked and as perfectly burnt as poſſible, will greatly contribute to the hardneſs of the cement, is evident from this con-

ſideration, that it is only when newly and perfectly burnt that all the lime is capable of hardening by expoſure. If it is kept for ever ſo ſhort a time after burning, ſome of it returns to its original ſtate. Thus a part of the lime is capable of hardening by attraction of air and cryſtallization, while a part of it is incapable of any alteration, and is of no more ſervice than an equal quantity of chalk or unburned lime. By theſe means a part of it is not only uſeleſs, but by ſcreening the perfect part from the action of the air, it retards the hardening of the cement and tends very much to make it ſcale off.

For theſe reaſons I think the common practice of keeping lime a long time before it is uſed is a very bad one. The workmen generally ſlake their lime, mix a quantity of ſand with it, and keep it in this manner for ſeveral months, and this proceſs they call by a very improper name, "ſouring of the lime." This practice is very general, and to it I am perſuaded in a great meaſure is owing the bad quality of the cement uſed in modern buildings. This can be of no ſervice but to undo a part of the effect produced by the heat upon the lime, as the long expoſure to the air will furniſh that matter which had been ſeparated by the fire. There is, however, generally ſome good reaſon for any practice which is common to all the perſons in the ſame trade, though it may not, in all caſes, be eaſy to diſcover it. And it will be found that though this practice will never make good cement, it may improve bad cement

made of half-burnt lime. This bad cuſtom ſeems to have been introduced by an erroneous opinion, very general, concerning the nature of the calcareous earth or limeſtone, uſed in making lime, to wit, that the pureſt limeſtone, or marble, or chalk, do not make the beſt cement, and workmen univerſally prefer lime made of the harder and more impure limeſtones. It is difficult to underſtand what firſt ſuggeſted this erroneous notion. It ſeems, however, to have been owing to the following circumſtances. Chalk, though the pureſt ſpecies of calcareous earth, is a very ſoft and ſpungy ſubſtance. When expoſed to the fire for a ſhort time it flakes very eaſily, though it is not above half burned. The more impure limeſtones, on the contrary, after calcination, will not fall into powder upon ſlaking unleſs the air has in a great meaſure been expelled by the fire. The ſlaking of the lime is, in the laſt caſe, a proof of the limeſtone being perfectly burnt; which is not the caſe in lime made of chalk. This might make the workmen aſcribe to the ſtone the qualities which depend intirely on the calcination.

The vulgar criterion, that the ready ſlaking of lime is a proof of its fitneſs for building, is true only in lime when made of hard ſtones. This ſeems to be the chief reaſon why all the authors, whom I have conſulted, from Vitruvius downwards, agree that lime made from the cloſeſt and hardeſt ſtone makes the beſt cement. Another reaſon for this opinion may be, that ſpungy ſubſtances, like chalk,

more eaſily recover the air, which the fire had ſeparated, and are more injured by keeping, than lime made of harder ſtones. A pound of chalk lime will abſorb 2¼ ounces of air in a day without apparently ſlaking. The lime of chalk may therefore be much injured without the ſlaking ſhewing that it is ſo. Some portion of every kind of limeſtone, uſed in this country, is not ſufficiently burnt, and does not ſlake readily. Some parts are half vitrified, and ſome limeſtones contain marl or gypſum. Workmen would naturally be led to the practice of keeping the lime ſome time after it was ſlaked, that the water might penetrate the whole more thoroughly. In the common lime, if this practice was not followed, it could ſcarcely be uſed as a cement at all, becauſe thoſe parts which eſcaped the action of the water in the ſlaking, would afterwards imbibe moiſture from the air, would ſwell, crack, and fall off. The practice of ſouring the lime, therefore anſwers no other purpoſe but to give time to the water to penetrate the whole of it; and as the beſt burnt is penetrated ſooneſt, while the reſt requires longer time, the whole is, by keeping, brought nearer to equality, but far ſhort of that ſtate of lime which is capable of giving the greateſt degree of hardneſs in cement.

There is, perhaps, another reaſon for ſouring lime. Plaſter for the inſide of houſes muſt be laid on very ſmooth, and requires to be more tenacious, and to have more lime in it than the cement uſed

in building. This quantity of lime would be apt to crack in drying, for the reaſons already mentioned. They are therefore obliged to keep it longer, that the lime, previous to being uſed, may have time to abſorb a quantity of air. The builders, conſidering the plaſterers' mortar as of a finer kind, think it not improper to imitate them in thoſe particulars, which are attended with no expence, and do not ſeem to know that ſuch meaſures prevent their cement from ever acquiring that degree of hardneſs in which its perfection conſiſts.

The proper method of uſing lime made of the coarſer limeſtones, and avoiding the inconveniencies ariſing from the unequal action of the fire, even in the beſt, would be to put the lime on a ſieve, plunge the whole in water for a few moments, lift it up, and allow all the lime to ſlake that will do ſo readily, and ſeparate this part carefully from the reſt. It will always be found to be the beſt calcined, and will produce a finer and harder cement than could have been made, had the whole been ſlaked in the ordinary manner. The part which does not ſlake ſhould be kept for other purpoſes, and the expence of this proceſs might be diminiſhed by employing that, which does not fall at firſt, as a manure for land, for which purpoſe it is as good as lime in its moſt perfect ſtate.

Beſides having lime in the beſt condition, it is neceſſary that the whole maſs of cement ſhould be ſo made that the lime in it may be expoſed to the

air as much as poſſible. The ſmaller the proportion of lime in cement, the more ſurface will it expoſe to the air. On this account, ſix or ſeven parts of ſand to one of lime, will be more convenient, than the common proportion of two or three. But ſo large a proportion as ſix or ſeven parts of ſand to one of lime, cannot be uſed for cement without the mixture becoming ſo brittle, that it will not adhere to the ſurface on which it is ſpread. If one was to mix flints or ſand in fine powder in this proportion with lime, the deſired tenacity might be given to the cement, but other inconveniences would be produced. The cement thus made is never ſo ſtrong as that made with coarſer ſand. It makes indeed a beautiful and ſufficiently hard cement for finiſhing the inſide of rooms, much harder and cloſer than the common ſtucco: but it is not hard enough to give all the firmneſs to the ſtones in a building that the beſt mixture of lime and ſand is capable of giving. If the cement made with powdered flint is ſtruck with a hard body, it cracks, and the fiſſure is always rectilinear, or nearly ſo. When mortar is made of coarſe ſand, it becomes much harder, and when ſtruck by a hard body the crack is never in a right line. The cauſes of the inferiority of cement containing fine powder ſeem to be two, the one mechanical, and the other chemical. The firſt is, that, a ſolid maſs of fine particles of unequal hardneſs being ſtruck, the ſofteſt are broken; and the ſize of the hardeſt being very ſmall, the direction

of the crack ſeems to be the ſame as if the whole had been made of ſoft matter, and the crack is in the ſtraight line. But if we ſtrike a maſs of cement, conſiſting of fine ſoft particles and coarſe hard ones, the fracture is always crooked, becauſe ſo much of the ſofter matter cannot be found in a ſtraight line, as to make the fiſſure rectilinear: the harder and larger particles are for ever ſtanding in the way, and the whole maſs is more difficult to break. The ſecond reaſon why the maſs with coarſe ſand and lime will be harder than fine powdered flint and lime, is, that the fine powder produces a more tenacious maſs, through which air and moiſture paſs with more difficulty. The lime in ſuch a mixture will therefore be longer in recovering the air it had loſt, and will be longer of aſſuming a cryſtalline form. On theſe accounts, perhaps, fine powder of flint in cements cannot be uſed where greater hardneſs is wanted. It may be employed in ornaments in the inſide of a houſe, but never to advantage as a cement in building.

Other methods muſt be tried to mix the ſeven parts of ſand with one of lime. The beſt mode of mixing ſo large a portion, ſeems to be, to uſe ſand of different ſizes in the ſame cement: the larger pieces will give ſtrength to the mixture, and allow the air more freely to come in contact with the lime; the finer ſand in the mixture will fill up ſome of the interſtices betwixt the particles of the coarſer, and with a leſs proportion of lime, give a greater degree of tenacity, than if the cement had been

made of either ſeparate. It ought further to be obſerved, that a little difference in the cement will ariſe from the ſhape of the particles of the ſand in it, as well as from its quantity. The cement will be harder when the particles of ſand are flat, than when they are round. When they are round the particles touch each other in fewer points. The coheſion of the whole will therefore be leſs. When the particles of the ſand are flat the ſurface in contact is greater, a ſmaller quantity of lime will fill up the interſtices, and leſs of the lime will be kept away from the action of the air. The cement will therefore not only become harder, but will do ſo in a ſhorter time, becauſe the lime in it will ſooner and more perfectly recover the air it had loſt in the fire.

That ſharp ſand anſwers better in the compoſition of cement than round ſea ſand, all builders acknowledge. But few of them will admit that ſeven parts of ſand to one of lime will produce a cement ſuperior to that in common uſe. That this proportion, however, is better than the common, may be proved by examining the cement in old buildings, where, from the age of the ſtructure and firmneſs of the cement, there is reaſon to ſuppoſe that it was of the beſt quality. In many old cements I have found by experiment the proportion of lime not more than one-ſeventh of the maſs; and that in ſuch cement the ſand or flint is of different ſizes, muſt have been obſerved by every body.

It ought likewiſe to be remarked, that a little latitude is allowable in the quantity of ſand and water in the compoſition of cement according to the time it is ſuppoſed the cement will take to dry. Much lime and as little water as poſſible will anſwer beſt, when the cement is to dry ſoon. Leſs lime and more fluid will anſwer beſt, if it is to dry ſlowly, for the ſame quantity of water gives more fluidity to mortar in proportion as the ſand is coarſer.

Though the mixture of ſand of different ſizes with lime and water in the proportions already mentioned, produce a good cement, it is very long before it acquires its utmoſt degree of hardneſs; and this is its greateſt fault. The older it is the harder it grows. But the firſt year it is liable to be penetrated by rains, and, if froſt ſucceeds, to ſcale off by the expanſion of water in freezing. The failing of the common cement, as well as that above-mentioned, is principally owing to this; to remedy which, many perſons have propoſed to uſe ſomething beſides ſand and lime in the compoſition of cement.

The moſt celebrated cement upon this principle was propoſed by M. Loriot, and conſiſted of a quantity of powdered unſlaked lime mixed with a cement made of ſand and common chalk or whiteing. The mixture was to be uſed immediately. But this compoſition, though very much extolled by its author, has not come into general uſe. He

ſeems to proceed on the miſtake that the drying and hardening of the cement are the ſame thing; though the one depends on the ſeparation of the water, and the other upon the abſorption of a quantity of air, and conſequent cryſtallization. M. Loriot's cement does, indeed, dry very faſt by the unſlaked lime ſoon abſorbing the water in the cement. But it is ſtill very far from poſſeſſing the hardneſs of old cements, and if the unſlaked lime is unequally burnt, it continues to ſwell, and in a ſhort time falls off, as I have found by repeated trials, as well as that the addition of old lime or chalk or whiteing is never of any advantage, and generally does harm. The proportion of ſand in Loriot's cement is too ſmall.

Others have recommended ſkimmed and butter-milk or blood to be uſed inſtead of water in mixture with lime and ſand in the compoſition of cement; and many are of opinion that theſe ſubſtances contribute to the quick hardening of the cement. But in all mortars which I have ſeen, they ſeemed to be of no ſervice, and always gave a diſagreeable appearance, when uſed in caſting of houſes, a vegetation being produced in the cement, which by keeping it moiſt tended to ſpoil it. Decoction of linſeed, though ſtrongly recommended, has the ſame inconveniences, and all of them produce no advantage, except making the cement more glutinous than it would have been with pure water; but it is

as liable to crack as the common cement, and the only ſervice they can be of is to give an equal tenacity to a leſs portion of lime in the mixture. Linſeed and olive oil mixed with lime produce a ſort of ſoap, which is long in drying, and ſeems to have no properties of a cement. Mixtures of linſeed oil, chalk, and ſand, in dry ſituations, acquire a great degree of hardneſs, and are ſometimes uſed as a cement in building, they differ little from common putty, but the oil ſpoils in time. Painting of cement prevents the further hardening by cutting off the communication with air, and wherever water can get behind the paint, irreparably deſtroys it. Charcoal of wood, and charred pitcoal powdered and mixed with lime and ſand, ſeem to make the cement harden faſter; but the difference is not very great: the cement by this mixture acquires a bluiſh grey or ſlate colour.

Some have recommended clay in the compoſition of cement containing lime and ſand. But in all mixtures of this kind the cement is much injured by the addition of clay. It is more diſpoſed to crack in drying, to ſoften in wet weather, and even to moulder, if the clay be one-eighth of the cement. In a ſmaller proportion it only prevents the mortar from acquiring the hardneſs peculiar to it, and diſpoſes it to periſh in a few years. Beſides clay adheres but very weakly to any hard bodies, however ſlowly it may be dried on them; and mixtures of

ſand and clay never acquire any conſiderable hardneſs by mere drying and expoſure to the air. As dried clay greedily imbibes water, and contracts greatly in the drying, it muſt be injurious in cement.

The ſubſtance moſt recommended to increaſe the hardneſs of cement is terra puzzolana: this is a volcanic product, conſiſting principally of clay hardened by fire and partially melted by a mixture of calx of iron. Its effects in the compoſition of cement are of little importance unleſs the cement be kept conſtantly wet. A cement made of powdered terras and lime was uſed by the Romans in all their aqueducts and buildings to contain or keep out water, and it has been generally employed in ſuch ſtructures ſince that time. When uſed in buildings which are conſtantly wet, it ſets very quickly, and is then impenetrable to water. From this it has been rather haſtily concluded, that it is the beſt ſubſtance that can be mixed in cements of all kinds. But cement containing terras does not become ſo hard, nor endure the weather ſo well as cement made of good lime and ſand in proper proportions; on the contrary, it is very apt to crack and periſh quickly in the open air. Its efficacy in water fences is only felt when it is kept always wet, and ſeems to depend on ſome properties which powdered terras have in common with all martial clays hardened and almoſt melted by fire. It abſorbs water and promotes the

cryſtallization or fixing of the calcareous earth, and by this means ſwells ſo as to render the cement impenetrable to more water. That terras n cements operates in the manner juſt mentioned, ſeems to be proved, by obſerving that burnt clay, if it contain iron, and all martial ochres, which conſiſt of iron and clay, have the ſame effect as terras in cement, and may be advantageouſly uſed inſtead of it. Water cement generally conſiſts of lime and terras, but no ſand, though this laſt ſeems to make the cement harder.

The ſubſtance which makes the beſt cement with lime, for the purpoſe of reſiſting water, is iron reduced to ſmall particles, either in the form of iron filings, boring duſt, or powdered ſcales of iron. Theſe mixed with cement made of lime and ſand in the proportion of $\frac{1}{14}$ part or leſs, ſeemed to be of no ſervice in dry ſituations; but in wet places, where cement made of lime and ſand will not harden at all, they were very ſerviceable, and by the cloſeneſs of the texture produced by the ruſting and ſwelling of the iron, excluded water perfectly. Mixed in a larger proportion with lime, ſuch as five or ſix parts of iron filings to one of lime, they produce a cement, which will reſiſt the ſteam of boiling water. A cement of this kind, which I ſaw lately, had reſiſted the force of the ſteam of boiling water for ſeveral months. Iron then, would probably, with lime in the compoſition of mortar, be a more effectual re-

medy for dampneſs in buildings than any ſubſtance hitherto uſed for that purpoſe. But in dry ſituations this does not anſwer, if the iron be more than one-fiftieth of the maſs of mortar.

A late author has propoſed to mix bone aſhes with the cement made of lime and ſand. One part of bone aſhes to four of lime, he ſays, improves the cement, makes it dry quicker, and not ſo apt to crack, without injuring its hardneſs much, while it improves the tenacity. A quantity of bone aſhes equal to that of the lime, is not ſo durable, but dries very faſt, and is not injured by hot weather. More would diminiſh the coheſion of the cement, and leſs would be of no ſervice. The bone aſhes in cement ſeem to be uſeful by their being porous, in conſequence of which they admit air and moiſture, and thus accelerate the ſetting of the cement, and ſecure the plaſter from moiſt and variable weather, and prevent dampneſs.

As the ſtrength and duration of our moſt uſeful and expenſive buildings depend on the goodneſs of the cement with which they are conſtructed, an inquiry into the nature of cement is of great importance, particularly in this country, where the weather is ſo variable and trying, and the common cement ſo bad. In moſt of our buildings the timbers laſt longer than the walls, unleſs the mouldering cement be frequently replaced by pointing. It has been generally ſuppoſed, that the Romans were acquainted with a cement far ſuperior to any now

known. In their moſt expoſed ſtructures, the aqueduct, it has withſtood even a trial of fifteen hundred or two thouſand years. But the nature of this cement ſhall be conſidered in another eſſay.

ESSAY XI.

ON ANCIENT CEMENT.

A. D. 1783.

Two kinds of cement have been found in the ruins of ancient buildings; one a bituminous, the other a calcareous cement. Bituminous cement conſiſts of aſphaltum or the bitumen judaicum: this ſubſtance is of a dark brown or black colour, of a ſoft or liquid conſiſtence, found in great quantities on the ſurface of certain lakes, in different parts of the world, particularly in the Dead Sea. Some ſpecies of aſphaltum are hard and ſolid, and the moſt fluid kinds become ſo by age. The thick and ſolid aſphaltum is employed at preſent in Egypt, Arabia, and Perſia, as pitch for ſhips; the more fluid ſpecies are uſed for burning in lamps. Vitruvius mentions, that Semiramis ſurrounded Babylon with a wall of brick cemented with a liquid bitumen, which was got in a very large lake in the neighbour-

hood, to which he gives the name of Limne Aſphaltidis. He ſays, there are large lakes, which yield liquid bitumen alſo at Joppa in Syria, and in Arabia. Galen mentions, that in Judea there was a lake which was called Aſphaltites, from the bitumen aſphaltum, which was found in the Dead Sea. The aſphaltum ſeems, in many reſpects, to be very well fitted for a cement in building. It is either fluid naturally, or may be made ſo by a ſmall degree of heat, and it becomes exceedingly hard and tenacious by age. Thoſe ſpecies which will not melt cannot be uſed for any purpoſe in building. But thoſe which are fluid, or which melt eaſily, are very convenient; they ſoon acquire all the hardneſs of which they are capable, are not injured by being ſuddenly dried, but become equally firm and durable in the air, and impenetrable by water. Theſe qualities of aſphaltum render it a very uſeful cement, and though it is in ſome reſpects inferior to the calcareous cements, it anſwers much better for many purpoſes. But it is found in but few places in ſufficient quantity to be employed in any large work. The Dead Sea furniſhes the greateſt abundance of it: it is found alſo in ſeveral parts of Europe, and a very little of it in Scotland. The diſtrict of Neufchatel is the only place in Europe that furniſhes a conſiderable quantity of it. This ſpecies is of the conſiſtence of honey, and is uſed as a cement for walls, pavements, water-courſes, and for many other purpoſes. It has been found to anſwer there ſo well,

that it cannot be doubted that the aſphaltum is a moſt excellent cement, well fitted for the purpoſes to which it was applied by the ancient inhabitants of the eaſt.

But it was only employed to join bricks or ſtones with flat ſurfaces; its fluidity rendered it inconvenient when the ſtones were not fitted to each other. Perhaps, it would be a uſeful ſubject of inquiry to ſearch after a ſubſtance, which might be ſubſtituted for the natural aſphaltum in building. Common pit-coal ſeems moſt likely to anſwer this purpoſe. It furniſhes by diſtillation an oil, and a ſubſtance like tar, which in its chemical properties, very exactly reſembles the natural aſphaltum, and like it is fluid at firſt, and becomes hard and firm by age. The ſubſtance like tar has been lately employed inſtead of pitch in the ſame manner as the natural aſphaltum was uſed in Perſia, and has been found ſuperior to common pitch for preſerving the bottom of ſhips. There is every reaſon to expect it would equal the natural aſphaltum, as a cement in building. Perhaps, its price would prevent it from coming into general uſe now, in the ſame manner as the ſcarcity of natural aſphaltum prevented it from being generally uſed by the ancients. The art of uſing aſphaltum, as a cement, cannot be ſaid to be loſt. It was uſed formerly, only where it was found in plenty, and where that is the caſe, it is uſed ſtill.

This cement, however, is not in general ſuperior

to the calcareous cement, and has ſome diſadvantages, which that other is free from. It is very dangerous in caſe of fire, though it loſes much of its inflammability and fuſibility by age. It never acquires the hardneſs of the beſt calcareous cement, and is chiefly ſuperior to it, with well-jointed brick and ſtone in thin walls, where it dries ſoon. It is not injured by moiſture, which ſpoils the calcareous cement. All fuſible and inflammable ſubſtances are liable to the ſame or greater objections.

Another cement was contrived, and much more generally uſed, in which lime was the cementing ingredient, and other ſubſtances were mixed with it, to remedy ſome inconveniences, which lime alone is ſubject to. A cement of this kind, has been uſed both by the ancients and moderns; but the ancient cement, it has been imagined by many, was ſuperior to that of the moderns. It has been ſuppoſed, that the ancients mixed ſome ingredient in their cement, which is now unknown; that this long loſt ingredient is the cauſe of the duration and hardneſs ſo much admired in ancient ſtructures; and that the art of making this ancient cement is a loſt art. But, as the moderns uſe a cement that very much reſembles the ancient one, many have doubted whether this be really inferior to the ancient in any other reſpect than what is merely accidental. It has been imagined, that it is not probable that any knowledge once acquired in any art ſhould be loſt. That this is leſs probable, if the

art was a useful one, and practised by different people in different countries; that it is highly improbable, that the knowledge of a useful ingredient should be blotted out of the memory of so many people. Besides it has been asserted, that the ancients had no art in making cement, and that therefore it could not be lost; that the so much admired cement, which is to be met with in those ancient Roman aqueducts, and other structures, which have stood so many ages, ought to be considered as monuments of good luck, and not proofs of superior skill in the composition. This they endeavour to prove by shewing, that it is only upon very rare occasions, that the ancients were able to make lasting structures. When many fortunate circumstances concurred, the building was lasting; when these were wanting, it was not more durable than a modern house. They succeeded only when quarries of good lime, free of gypsum, were found near the building to be erected; when the limestone was very hard, and would not slake unless it was well burnt; when the building was at the public expence, and where there was no temptation to be parsimonious in the fuel; when the number of workmen employed was so great, that the lime was used as fast as it could be made, and when it ran no risk of being spoiled before using by long exposure to the air; and when the sand in the neighbourhood was clean, sharp, and well sized, resembling a mixture of coarse and fine sand. In such situations it has

been aſſerted, that it was not in the power of ignorance to make bad cement, and that chance furniſhed all that ſkill could deſire.

The faſhion of ancient buildings, it has been alſo ſaid, gave their cement many advantages. The walls were of an immenſe thickneſs, intended to be ſupported by their own ſtrength, and not held together by timber like modern buildings. This great thickneſs ſecured them from drying too faſt; their great weight ſecured them from being ſhaken, and contributed to make the coheſion greater by bringing the different pieces, of which the wall was compoſed, into cloſer contact. The great thickneſs of the wall alſo took up much cement, and the quantity was greater than in modern buildings, becauſe the walls conſiſted of ſmall ſtones with mortar run between them. As the quantity of lime thus became immenſe, every method of ſaving it was of importance. Motives of economy led them to uſe as little lime and as much ſand as poſſible; this, and not ſuperior ſkill, led them to uſe the very beſt proportion of lime and ſand in the compoſition of their cement.

From all theſe circumſtances, it has been concluded that accident gave ſuperiority to ancient cement, and put it in the power of any body to make it. Many other circumſtances might be brought to prove the ſame thing, and that the ancients had no fixed rules to direct them in the compoſition of cement, and that its quality depended very much on accident. It might be ſaid, that all old cement

is not good; ſome of it is bad, and crumbles eaſily between the fingers. In the ruins of Herculaneum, and other ancient cities, much bad cement has been found. All ancient buildings have not laſted equally well, and we ought not to aſcribe the duration of the few that remain to ſuperior ſkill.

It may be ſaid, that the preſervation of the few, has been owing to accident, rather than ſuperior ſkill; that many circumſtances had conſpired to make the cement good, or that the building had been taken great care of, and the injuries of time prevented or repaired. Beſides, though it be admitted that there was a ſecret in the compoſition of ancient cement, and that this ſecret has been loſt, yet no body has been able to determine when this loſs happened. The common practice of the admirers of the ancients, is to allow it to have been known in all the ſtructures in which the cement is now found to be good, and to deny that it was known in thoſe whoſe cement is of an inferior quality. Every body allows it to have been known even in the decline of the Roman empire. Moſt people alſo allow it to have been known to the Saracens. In many Saracen or Gothic buildings the cement is not inferior to that in the beſt Roman buildings, as is evident in many old caſtles and religious houſes built ſince the conqueſt. If we admit that the hardneſs of the cement in the one caſe is owing to ſkill in making it, we muſt alſo admit it in the other. We may, indeed, ſuppoſe the ſupe-

rior quality of the cement, both in Roman and Saracen buildings, merely the conſequence of time. We may endeavour to prove this, by examining the cement in houſes built ſince the reformation, in many of which it is of the very beſt quality. If there was any ſecret in the matter, it muſt have been known at the reformation, and it is hardly poſſible to conceive, that an æra remarkable for the revival of learning, ſhould have been attended with the loſs of a ſecret ſo generally known and ſo generally uſeful. The ſuperiority of ancient cement, it has been concluded, was either accidental or the mere effect of age, and was not owing to ſuperior ſkill in an art, which is now loſt.

But though theſe arguments give ſome probability to the opinion, that the art of making the cement uſed by the ancients is not loſt, yet they by no means put it out of doubt, and ſeveral circumſtances ſeem to ſhew, that if the art is not loſt, it is no longer practiſed; that there is a great difference in the appearance of ancient and modern cement, and that it is not improbable that this may be attended with a difference in hardneſs and durability, unleſs where accident has brought the cement in modern buildings nearer the ancient. Ancient cement contains a ſmaller proportion of lime than that now in common uſe. It contains particles of different ſizes, ſome as large as nutmegs: the modern cement contains no particles coarſer than ſand.

It will hardly be admitted, that the ancient cement

was good only by accident. It, on the contrary, is very uniform in its ſtructure, and differs regularly in its appearance from the modern. This conſtant difference cannot be owing to chance. It is more probable, that this difference in the compoſition, is the reaſon why the ancient cement is ſuperior t the modern. It ſeems to be but reaſonable to allow, that the ancients had diſcovered that this compoſition gave to their cement greater hardneſs and durability. And this becomes more probable, when we find from Vitruvius, that the ancients had a cement like ours, which they uſed only for ornament or in the inſide of houſes. This is what they called the opus arenatum, but it is not ſuperior to modern cements. The ſpecies of cement, which they employed in building their Cæmentum, was uniformly different from our mortar. It differs in containing leſs lime with a mixture of coarſe and fine ſand. When ſand conſiſting of a proper aſſortment of coarſe and fine particles could not be got, they ſubſtituted coarſe powdered brick for it, and produced a very good cement: the ſigninum, according to Pliny. This mixture of coarſe particles gave greater ſtrength and cohefion to the mortar, made it more difficult to break in any one direction, and better calculated to reſiſt the weather. This contrivance alſo enabled them to mix more ſand with lime without injuring its plaſticity. The mixture of coarſe and fine ſand is not uſed by the moderns, becauſe it would be more difficult to manage,

and more of it would be neceſſary than of the preſent cement. The greater quantity of cement, and the greater labour in uſing it, ſeem in the opinion of the ancients to have been compenſated by its hardneſs and durability. This then is one fixed principle in the compoſition of ancient cement, and probably one cauſe of its ſuperior quality.

Another difference between ancient and modern cement, is that the modern is often made many weeks and even months before it is uſed. This injures the cement, prevents its ever acquiring a great degree of hardneſs, but makes it ſmoother and more plaſtic, and ſaves time in uſing it and mortar. This ſeems to be the great principle, to which every thing in modern buildings is ſacrificed. In ancient walls, conſiſting of ſmall ſtones run together with mortar, this could not have been uſed at all. The ancients, as we are informed by Vitruvius, knew this method of touring mortar; it was uſed only for the Opera Albaria, or in the inſide of buildings; but the proportion of lime is too great in it ever to acquire the hardneſs of ancient cement.

Both good and bad cement are to be met with in ancient buildings, but this is not a proof that the ancients had no fixed principles in its compoſition. It might be owing to ſeveral cauſes. Good lime and ſharp ſand are not every where to be met with, and even when they were, they might not be uſed in the proper proportion from the careleſſneſs of workmen. Even when none of theſe cauſes ope-

rated, many other circumſtances might hurt it. The walls might be injured by rain, or ſhaken by wind while the cement was hardening. All the old Roman houſes were not equally well built. Their public buildings were either of hewn ſtone, and the cement ſmooth, a mixture of lime and ſand, or were cementitious buildings conſiſting of ſmall ſtones run together with mortar, more durable. Many of the old Roman houſes, for lodging, were built of brick dried in the ſun, and cemented with clay mixed with ſtraw. In ſome of the provinces the law did not allow theſe bricks to be uſed unleſs they were five years old: one could not expect theſe buildings to be durable. Vitruvius informs us that the laws forbid any houſe to be built, within the city, of brick, whoſe walls were more than 1½ feet in the thickneſs, and that not more than one ſtory could be raiſed on them, for fear of their falling in pieces by their own weight; that the number of citizens and majeſty of the city did not permit ſuch buildings. Without the walls they were allowed to build what houſes of brick they pleaſed. Accordingly, within the city the houſes were of ſtone, the walls very thick, many ſtories raiſed above one another; Rome was thus confined in leſs ſpace, its citizens were better lodged, and the ſtreets and public places left more ſpacious. Theſe circumſtances would produce great difference in the cement of ancient buildings. It has been ſaid that a great deal of bad cement has been found in the ruins of Herculaneum. But in that

in Vitruvius, and may ſee their effect in old buildings, in which, by chemical experiments, we can diſcover the exact proportions he recommends. But though the ſecret of ancient cement is not loſt, the art of making it is not practiſed in this country.

It is difficult to find out a reaſon why ancient cement was laid aſide, and one ſo far inferior ſubſtituted for it; or when this change took place. Perhaps it might be owing to ſtone buildings cemented with lime being much leſs common at the time this change happened than at preſent. Formerly well cemented ſtone buildings were either public edifices, or the ſeats of the chief nobility. They were not only intended for lodging, but were places of defence. No expence was ſpared to make them as ſtrong as poſſible. Their walls were thick and maſſy; they required an age to build. The walls were cementitious, in the manner of the ancient Roman buildings, and their cement has every property of the ancient cement. But though perſons of higher rank were lodged in houſes whoſe cement was not inferior to the old Roman, the middling and lower orders were lodged in very mean habitations. Their houſes of ſtone were not ſo well built as the walls that incloſe our fields, and the wind and rain were imperfectly kept out by a periſhable cement of clay and ſand. The greateſt part of their houſes were of turf, and a very few of wood. While property was not ſecure, and when the common people had little liberty, they never thought of, and were

incapable of affording more durable and expenſive houſes. The demand for ſubſtantial ſtone ſtructures was ſo ſmall, that two or three maſter-builders were ſufficient for a whole kingdom. When liberty increaſed, and property was more ſecure, riches accumulated among the lower ranks; they then began to wiſh for better lodging than their miſerable huts afforded. The building a houſe became a mark of riches and ſucceſsful induſtry, and helped to tranſmit the name of the builder to his poſterity. This ſpecies of property was then better ſuited to the circumſtances of ordinary people, than property in land, and could he more eaſily obtained. They wiſhed for ſomething more ſubſtantial and leſs dangerous than wood, and cheaper than the maſſy and diſmal dwellings of the great. A ſpecies of building ſuited to their circumſtances was contrived, and the expence was diminiſhed by raiſing thinner walls, and making the mortar in great quantities at once, and of a ſmoother conſiſtence, exactly like the *opus arenatum* of the Romans. By this means leſs mortar ſerved, and labour was ſaved. The demand for theſe ſlighter buildings becoming great, the trade fell into more hands, and inſtead of two or three builders in a kingdom, half a dozen could be met with in every town. Every one ſtrove to make houſes cheaper and ſlighter than his neighbour, without injuring their appearance, and houſes could be built of any dimenſions, at any price, and to laſt any number of years deſired. This ſlight ſpecies

of building became ſo general, that the ancient maſſy walls built with a great quantity of ſtrong cement were laid aſide, and are ſo little in requeſt at preſent that very few workmen know any thing about the manner of conſtructing them. It is, indeed, ſcarce poſſible to find one who will alter the mode of building to which he has been bred, even when a maſſy, durable, and expenſive public edifice is to be erected.

ESSAY XII.

ON THE DIAMOND.

THE diamond is the hardeſt of all minerals. Its ſpecific gravity is nearly 3.5. When brought into Europe in its rough ſtate, it is either in the form of round pebbles, or in cryſtals of an octohedral form. Sometimes it is met with like hexagonal priſms terminated by eightſided pyramids, or in regular cubical cryſtals with the angles truncated, or cut off. Whether theſe differences be original or adventitious, has not been determined. Thoſe in the form of hexagonal priſms, are commonly reckoned the hardeſt, and may therefore have preſerved themſelves againſt accidents better than thoſe that are rounded, which are ſaid to be the leaſt hard, and of conſequence, more liable to be altered by friction upon other ſubſtances. The round diamonds may be ſuppoſed to have undergone the ſame changes with many of

the common rock cryſtals that have been torn from their beds by changes in the earth, and by agitation with water ground and rubbed againſt one another, till they have been reduced to a round figure.

It is not known whether there is any rock where the diamonds are found, which has the properties of theſe precious ſtones, or, which has ſerved as a baſis or matrix for their formation, in the ſame manner as quartz is the baſis of rock cryſtal. The account which travellers have given us of the natural ſituation of diamonds in Golconda and Viſapore is, that they are found ſeparate at a conſiderable diſtance from each other, but no mention is made of the quality of the rock in the neighbourhood of the diamond mines. When the miners meet with rock before they can get to the ſtratum of earth where the diamonds lie, it would appear from their manner of working, that there is nothing remarkable in its qualities. The properties of the diamond are ſo ſingular, that had the rock in the neighbourhood poſſeſſed any of its qualities, a part of it would before now have been ſent into Europe, or examined and deſcribed by the travellers who have viſited theſe mines.

Diamonds are found in Golconda and Viſapore, in the iſland of Borneo, and lately in the Brazils. The continent of India, both on this ſide and beyond the Ganges, is remarkable for its mines. The diamond mines are generally near rocky hills and mountains. A chain of theſe begins near Cape

Comorin, extends in breadth near fifty miles, and runs quite through Bengal. In, among, and near theſe hills there are many mines known; but the barrenneſs of the country, the want of water, the badneſs of the roads, and the ſavage ſtate of the original inhabitants, who ſtill poſſeſs this part of the country, and who have little commerce with their neighbours, together with the tyranny of their petty princes have prevented theſe mines from being properly examined, ſo that a great part of them is concealed or wrought with great ſecrecy, leſt their produce ſhould fall into the hands of their rapacious rulers. But the kingdoms of Golconda and Viſapore have a ſufficient quantity of diamond mines to furniſh all the world plentifully with theſe gems. Their kings however, permit digging only in a few parts, leſt, as it is ſaid, the diamonds ſhould become too common, and in conſequence of this, diminiſh in their value.

All ſtones above a certain ſize are their king's property, ſo that few of extraordinary magnitude are brought to Europe. Diamonds do not lie in cluſters; they are found in detached pieces at a greater or leſs diſtance, according to their ſize. Thoſe mines which yield large ſtones, frequently do not contain above one in a quarter of an acre; thoſe that yield ſmall ſtones may contain a great number in the ſame ſpace. There are about twenty-three diamond mines in Golconda and fifteen in Viſapore. In theſe, the diamonds are commonly

found in a ſtratum of reddiſh earth, in many places, not unlike the gravel pits in Britain, which ſtains the miners' clothes, and appears not to differ much in its qualities from the red clays, or common iron ore of this country. One mine in Golconda and one in Viſapore have this ſtratum of a yellowiſh earth, and one in Golconda of a blackiſh earth. The depth of theſe mines is commonly three or four fathoms. They ſeldom can dig deeper on account of the water, which they have no method of removing. In one of the Golconda mines, they are obliged to dig through rock five or ſix feet deep before they come to the ſtratum of earth in which the diamonds lie. This mine is between forty and fifty fathom deep. They work through this rock with great difficulty, as they ſeem to be altogether unacquainted with the uſe of gunpowder in mining. They, however, in ſome meaſure, ſupply its place by lighting a large fire on that part of the rock through which they intend to paſs: when the heat has penetrated the rock to a ſufficient dept, they ſplit it by throwing cold water upon it. In one place, the diamonds are found in a ſmall vein of reddiſh earth, about half an inch or an inch in breadth, running irregularly through the rock. This earth, upon being pulled out, is found to contain diamonds. In the kingdom of Bengal, and in the iſland of Borneo, diamonds are found mixed with the ſand of rivers. Some of the ſtratum of earth in which theſe ſtones lie, is waſhed down from the ſides of hills by the rains, which fall in great quan-

tity at a particular ſeaſon of the year. Thoſe who ſearch theſe rivers for diamonds, prefer the parts where the heavier kinds of earth, particularly the pyrites have been depoſited. They begin their operations in the end of January or beginning of February, immediately after the rains are over.

The earthy matter in which the diamonds are found, covers them ſo perfectly, that it is with the utmoſt difficulty that they can be diſtinguiſhed from an indurated iron ore. It adheres ſo ſtrongly to them, upon many occaſions, that it cannot be got off, but by grinding or rubbing them againſt one another. From this, as well as from the foreign matter that is frequently found in the centre of diamonds, it would appear, that they were once in a ſoft or glutinous ſtate, or that it was not till long after their firſt formation, that they acquired that hardneſs, for which they are ſo much valued. In conſequence of the diamond, when bedded in the earth, having ſo little of the luſtre by which it is ſo eaſily diſtinguiſhed in its poliſhed ſtate, it is neceſſary to examine the earth in which they lie with the utmoſt attention. In order to render this as eaſy as poſſible, the lighter earths and thoſe parts that approach in quality to common mould are removed by agitating the whole with water, and pouring off immediately the muddy fluid. As the diamond is among the heavieſt of the earthy ſubſtances, there is little danger of its being carried off in this manner.

Thoſe parts which are not carried off by the water, are dried in the ſun, and carried to a ſquare place incloſed by a high wall, and carefully examined. Every thing that has the ſmalleſt reſemblance to the diamond, is rubbed againſt the common kinds of ſtone. If it does not diſcover itſelf to the ſatisfaction of the miners by this method, they ſtrike it with violence againſt a hard body, and ſometimes with the iron inſtrument with which they work. By this imprudent method, many of the fineſt diamonds have ſuch cracks or flaws formed in them, that their value is greatly diminiſhed.

The value of the diamond is aſcertained by weights called carats, each of which is divided into four grains, and theſe grains again into fractional parts. The ſmalleſt diamonds, in the ſtate in which they are dug up, are ſeldom leſs than $\frac{1}{4}$ of a carat or $\frac{1}{4}$ of a grain. The largeſt diamond known, is a rough one, in the poſſeſſion of the Portugueſe crown, which weighs no leſs than 1680 carats, or eleven ounces twenty-eight grains, nearly. There are ſeveral other diamonds of extraordinary ſize in Europe, but not at all approaching to this. Rough diamonds are valued at 2l. for the firſt carat, by the beſt authors, and afterwards their value is computed by multiplying the ſquare of the number, expreſſing their weight in carats by 2, and the product is the value in pounds ſterling. But this price is obviouſly too high for large diamonds, for which no purchaſers would ever be found at theſe extravagant rates. The

luſtre, the tranſparency, and the figure of the diamond alſo affect its value very much.

The diamond, as it comes out of the mine, is covered with a dark obſcure ruſt, which ſcarcely ſhews any thing of the brilliancy for which the ſtone is valued. In this ſtate the beſt connoiſeurs cannot judge of their value. The cruſt cannot be removed by rubbing with the hardeſt ſubſtances. No other method will anſwer but rubbing one diamond againſt another. The ſmall particles that are thus torn off are carefully collected, and uſed for the cutting and poliſhing of other diamonds, as well as for cutting all other ſtones. When the external cruſt, which appears to be in a different ſtate from the internal parts, is removed, the quality of the ſtone can be determined. Many that before this appeared to be good, are found to be full of cracks or flaws, which either were originally formed in them, or had been produced by the rude treatment which they frequently meet with from the miners. By removing the ſuperficial cruſt, many imperfections are perceived which could not be obſerved in their rough ſtate: ſuch as black ſpecks, which are frequently formed in their centre, and which ſeem to be of the nature of the diamond, but in a leſs perfect ſtate; and ſometimes materials totally different from the diamond are ſurrounded by a cruſt of that ſubſtance.

The colour of the diamond varies prodigiouſly. We find them, from being perfectly colourleſs and tranſparent, of various ſhades of red, yellow, green,

and even black. Theſe colours, however, are never of a very deep tinge. We never meet with them of a deep red, yellow, or green. They only have a faint tinge of theſe colours. The diamond that is perfectly tranſparent like the rock cryſtal, is more valued than any of the reſt, and next to them, thoſe that have a faint green tinge, but quite tranſparent, are preferred. Thoſe of a reddiſh or browniſh colour, or opaque, are the worſt. It is commonly very difficult to diſcover the true tinge of this ſtone, eſpecially if it has been cut in the brilliant faſhion, that is, if it has been cut to a number of points by a number of planes variouſly inclined to each other. For it poſſeſſes ſo great a refractive power, that if the reflection of the light, which would have paſſed through it, be prevented by a black foil placed below it, ſo that no light comes to the eye, but what is reflected from its ſurface, this light paſſing through the planes variouſly inclined to one another, and which act as ſo many priſms, is, by the high diſperſing as well as high refractive power of the diamond, ſeparated into as many ſpectrums as there are planes; and theſe, if there is the leaſt motion in the ſtone or undulation in the light ſo dazzle the eye, that it is impoſſible to diſtinguiſh the light which the diamond is diſpoſed to reflect from that which is ſeparated into the colours by the diamond itſelf in conſequence of its priſmatic figure.

The diamond is of a laminated texture. Of this the diamond-cutters are extremely ſenſible, as they

find it much eaſier to ſplit them in one direction than in another. This direction they are obliged to attend to in the poliſhing. It is eaſy to poliſh diamonds in the direction of their plates or fibres; it is impoſſible to poliſh them in a direction different from this. The ſame thing is evident from the manner in which diamonds cut glaſs. It is only in the direction of their plates that they do this. If the diamond be drawn along the glaſs contrary to this, it ſcratches the glaſs, but does not ſplit it.

It has been imagined, that the diamond does not differ in the nature of the earth of which it is compoſed from rock cryſtal: that it owes its great hardneſs to the heat of the climate in which it is found, and to its being removed from the rock which ſerved for its baſis. This has been imagined, from its being obſerved that rock cryſtals which have never been adherent to the rock, but which have been cryſtallized in their own bed or matrix, are always the cleareſt, and next to them, thoſe which, by ſome unknown accident in nature, have been ſeparated from their baſis. Whereas, thoſe that adhere to the rock are always impure and ſeldom good for any thing. But if the qualities of diamonds were different from thoſe of rock cryſtals in conſequence only of being removed to a greater diſtance from the matrix that ſerved for their formation and cryſtallized in a warmer climate, we ſhould ſurely meet with rock cryſtals in ſtates of hardneſs and luſtre inter-

mediate between them in their ordinary form, and the diamonds according to the climate in which they are found. But the rock cryſtal which is found on the coaſt of Barbary is equally inferior to the diamond in all reſpects, that the cryſtals in Lapland are. Others, with more probability, have imagined that all the other commonly reputed precious ſtones, ſuch as the ruby, the ſapphire, the topaz, and the emerald, were diamonds in an imperfect ſtate, or which failed in being diamonds by the mixture of ſome metallic ſubſtance. In conſequence of this, it has been aſſerted, that could we ſeparate the colouring matter from theſe ſtones, they would not be diſtinguiſhed from the diamond. On this account, ſome of the beſt naturaliſts, even Cronſtadt himſelf, call the ruby the adamas ruber. It differs but little from the diamond in hardneſs and weight. Others call the ſapphire the blue diamond. It is even pretended, that the diamond is found in a ſoft ſtate, as if it had not remained long enough in the earth, ſince its firſt formation, to acquire the hardneſs it is capable of acquiring. To this ſubſtance, the Engliſh jewellers have given the name of jargon. The natural ſhape of this ſubſtance is not known. It is ſaid that it is found in the Eaſt Indies, in the form of pebbles; that it is there ſplit into pieces and ſent into Europe; that it is of different colours, white, light yellow, and brown. According to ſome lapidaries, it comes neareſt to the ſapphire in hardneſs.

As theſe ſtones reſemble ſome diamonds in colour, they are called ſoft diamonds; and it is ſaid, that many have been impoſed upon, and purchaſed them for true diamonds. They, however, have none of that luſtre which depends on the high refractive power of the diamond, and reſemble more the topaz in this reſpect.

The colouring principle of many of the precious ſtones, and the colour of the diamond on ſome occaſions, has been commonly believed to be metallic: but this is far from being univerſally the caſe. We know indeed, that glaſs by the proper mixture of metallic calces, may be made to imitate the colour of all of them pretty exactly. But, it is at the ſame time well known, that the colour of the artificial ſtones is, for the moſt part, of a much more fixed kind than that of the native gems, and that they ſuſtain a degree of heat without injury, which entirely diſſipates the colouring principle of moſt of the gems. The ruby, however, which approaches ſo nearly to the diamond in hardneſs and ſpecific gravity, and which agrees exactly in its figure, cannot be deprived entirely of its colour in the moſt violent degrees of heat. While the ſapphire and the other coloured precious ſtones loſe their colours in a very gentle heat.

Reaſoning from analogy, joined to an univerſal diſpoſition in mankind to appear more wiſe than they really are, has greatly retarded the progreſs of natural knowledge. It is in this way, that ſome

naturaliſts, truſting to external form for the diſtinguiſhing mark, have imagined that the diamond and other precious ſtones are not materially different from rock cryſtals. While others, truſting to hardneſs as the mark, have inſiſted that the gems are all formed of the ſame earth, but by different mixtures with other ſubſtances, or by accident, have failed of arriving at their greateſt degree of perfection. The native gems, and particularly the diamond, have by univerſal conſent in all poliſhed nations in all ages, been conſidered as the greateſt marks of luxury and opulence, and have procured to their poſſeſſors a great degree of reſpect and admiration from the generality of mankind. It is not eaſy even for a philoſopher to diveſt himſelf of this univerſal prejudice in their favour. He is almoſt ſorry when any experiments oblige him to conſider them as little different from the ordinary ſubſtances that cover this globe, and he is apt to lay hold of the moſt trifling circumſtance to prevent their degradation: while they are ſo precious, that it is ſeldom in his power to examine them by all poſſible experiments. The chemiſts, who adopted the principles of the peripatetics, and who are diſpoſed to account for the formation of all bodies from four principles, of fire, water, air, and earth, imagined that the diamond, on account of its hardneſs and tranſparency, was the pure elementary earth. Theſe principles, however, are merely hypothetical. We cannot by experiment reduce all bodies to them,

and ſome of the principles themſelves are capable of being decompoſed. Unleſs from the general eſtimation in which the diamond is held, it is difficult to find why it was imagined to be the pure elementary earth.

I think it can be demonſtrated, as well as any thing can be demonſtrated by experiment, that it is far from being a pure earthy body. It is obſerved of the diamond, that it acquires the property of ſhining in the dark, in conſequence of its being expoſed to the ſun's rays. This property moſt of them poſſeſs, though it is obſerved by Du Fay, that one very fine diamond had not this property in the leaſt degree. The light which the diamond emits is very faint and of ſhort duration: it cannot be perceived unleſs the perſon who is obſerving it has remained in a dark room, till the pupil is dilated as much as poſſible. This property of the diamond, as well as its attracting light bodies, is generally believed to be of the electrical kind. It has been long known, that the diamond is incapable of fuſion. It has been expoſed to the moſt violent degrees of heat in furnaces, and by the Grand Duke of Tuſcany to the focus of Tſchirnhauſen's ſpeculum, both by itſelf and in mixture with alkaline ſalts, flints, metallic ſubſtances, and aſhes. It was conſtantly found, that whatever other alterations it might undergo, it never ſhewed the ſmalleſt diſpoſition to fuſion; and what was more remarkable, it was always more altered in its qualities when expoſed alone than when mixed

with other ſubſtances. The ruby expoſed to the focus of the ſame mirror for forty-five minutes, became in a little time ſhining, as if it had been rubbed with melted greaſe: air bubbles formed upon its ſurface; it loſt conſiderable part of its colour; its angles became blunt; and it acquired ſuch a degree of ſoftneſs, that it received the impreſſion of a ſeal, and was eaſily cut or indented with a knife. In theſe experiments, however, as well as in thoſe made by order of the Emperor Francis the Firſt, in violent heats raiſed by means of fuel, it was found, that of all the precious ſtones, the diamond was moſt affected by the heat. If they were coloured, when expoſed to the focus of the ſpeculum, they ſoon became colourleſs, loſt their brilliancy, and became opaque; air bubbles formed on their ſurface, and they loſt conſiderable part of their weight, and at laſt were entirely diſſipated. While the ruby in the ſame degree of heat loſt nothing of its weight, nor changed the leaſt in its qualities, the ſapphire, though it loſt all or the greateſt part of its colour, loſt none of its weight; and the emerald, though it melted, loſt little of its colour, and none of its weight. One would have imagined that ſome attention would have been paid to theſe experiments, in the arrangement of the precious ſtones, as they clearly prove that they differ conſiderably from one another. But none ſeems ever to have been paid to them, till the inquiry into the nature of the precious ſtones was lately revived by ſome of the French chemiſts. The

Chevalier D'Arcet was the firſt who repeated the experiments of Francis the Firſt, in a furnace for baking porcelaine, and he found that the diamond really evaporated. He obſerved, that this happened in a very moderate heat; that there was no occaſion, in order to diſcover this, to expoſe it to the focus of mirrors or lenſes, nor even to ſuch violent degrees of heat as were raiſed in the furnaces of Francis the Firſt: that all that was neceſſary for this purpoſe, was to heat them red hot. He ſatisfied himſelf of this, by expoſing them firſt to heat in open veſſels, and he afterwards varied the experiment by ſurrounding the diamond with porcelaine paſte, and cloſing all the openings with the ſame paſte diluted with water. The porcelaine balls including the diamond were put into the porcelaine furnace, and expoſed to the degree of heat neceſſary to bake the porcelaine. In ſome of the balls the diamonds remained unaltered. In others, they had loſt more than half their weight; in others, they were entirely diſſipated, without the ſmalleſt mark or flaw being perceived in the porcelaine cruſt. Theſe differences, M. D'Arcet was diſpoſed to account for, from the different nature of the diamonds made uſe of in the experiments; to ſome of them being Brazil diamonds and other Eaſt Indian ones. From theſe experiments, it was concluded, that the diamond might be diſtilled, and by this means, a number of ſmall diamonds put into a retort and diſtilled, might be united ſo as to produce a large one. Lapidaries and jewellers inſiſt, that

diamonds may be expoſed in cloſe veſſels to heat without loſing any thing of their weight. When they meet with brown or blackiſh diamonds, they frequently alter their colour, and render them paler by expoſing them to a conſiderable degree of heat. By theſe means they diminiſh their imperfections and increaſe their value. Macquer and ſome other chymiſts have repeated theſe experiments of D'Arcet on the diamond: they all agree, that in veſſels that are perfectly cloſe, nothing diſtils from the diamond, nor are its qualities altered in this ſituation in the leaſt. It has been put into an earthen retort with a receiver luted to it, and expoſed to the moſt violent degrees of heat that can be raiſed by means of fuel, without alteration. It has been ſurrounded with charcoal duſt and expoſed in a covered crucible to the moſt intenſe heat without loſing either its poliſh or its tranſparency, without loſing any thing of its weight or producing the leaſt alteration in the charcoal in contact with it, or ſhewing the leaſt appearance of fuſion. When expoſed to heat in contact with chalk and calcined hartſhorn, it loſes a little of its weight and ſometimes its tranſparency, ſo as to reſemble a rough diamond. The reſult of theſe experiments is different from thoſe of D'Arcet, the cauſe of which is, that in his experiments, the diamond was put into porcelaine paſte, which requires a great heat to bake it into china. For a long time it is in a ſpongy ſtate; during this time the external air might come in contact with the diamond,

and contribute to its diſſipation long before the heat had become ſo great as to render the porcelaine cruſt perfectly firm and ſolid.

I made an experiment lately on the effect of heat on the diamond. The degree of heat to which it was expoſed was about as great as is neceſſary to cupel ſilver. The diamond was put into a cupel in an aſſay furnace. It was about $\frac{3}{10}$ of an inch long, and $\frac{1}{8}$ of an inch broad. It had remained but a ſhort time in this ſituation, when a part in the middle of the diamond became apparently more prominent than the reſt. The diamond was manifeſtly more luminous than the cupel or muffle that contained it, and a gentle undulating phoſphorine flame was perceived on it, which was greater when a greater quantity of air was admitted into the muffle. After an hour's expoſure in this manner, it was taken out and examined. It was reduced to about one half of its former ſize, had become white, and loſt its tranſparency, and ſeemed as if the plates of which it is compoſed, had been ſeparated from each other and reſembled a rough diamond. It was returned to the fire, and kept above three hours longer in a leſs heat, during which, it gradually diminiſhed in ſize, till it was not above the ſize of the head of the ſmalleſt pin. Probably, had the heat been a little greater, or continued ſome time longer, it might have been entirely diſſipated. A ruby nearly of the ſize of the diamond was expoſed

along with it, but was not altered in the leaſt. Mr. Macquer and M. Mitouard agree in having perceived, that it manifeſtly emitted flame when expoſed to heat in the open air and diſſipated entirely. The French chemiſts expoſed a great variety of other precious ſtones in the ſame circumſtances with the diamond. Theſe ſometimes loſt their colour, became ſoft, or even melted, but none of them loſt any thing of their weight. Theſe experiments point out a manifeſt difference between the diamond and the other earthy ſubſtances, and ſhew that it reſembles the inflammable or metallic bodies more than the earths.

The diamond is inflammable: it burns in the open air, without leaving any aſhes, or emitting any ſmoke that can be perceived. It ſeems to be among the pureſt of the inflammable ſubſtances. With theſe it agrees in its great refractive power, and in ſuffering no alteration by being expoſed to heat, provided the air be excluded. It is not a volatile body, but is diſſipated merely in conſequence of its inflammation, in the ſame manner as charcoal, and the more fixed inflammables are. It is difficult without more experiments to ſay to what claſs of the inflammables it belongs, or whether it be a mean betwixt an earth and them. It reſembles the earths in hardneſs, but it differs from them in producing flame by heat, and diſſipating as it burns; while the flame which it produces is ſmall, brilliant, weaker than that

emitted by any inflammable when burning, and reſembling more the luminous appearance of the phoſphorus in the ordinary heat of the air, or the light emitted by tin or biſmuth when heated a little above their melting point.

NOTE BY THE EDITOR.

To the information contained in this account of the natural and chemical properties of the diamond the experiments and obſervations of thirty ſucceeding years have made a few additions, which are altogether corroborative of the opinion of the nature of that curious mineral, delivered in the latter part of this eſſay. It is now clearly aſcertained, that the diamond is of a carbonaceous nature. Lavoiſier, by means of a large burning lens, conſumed four grains and a half of a diamond under a receiver containing 60 cubic inches of air. During the proceſs, the air ſuffered a diminution of nearly $\frac{1}{30}$, and the remainder precipitated the lime from lime water; that is to ſay, carbonic acid was formed by the union of the diamond and oxygen gas. But charcoal forms the very ſame product with oxygenous gas, and therefore the diamond and charcoal are either the ſame ſubſtance, or contain the ſame ſubſtance. It is aſcertained, however, that diamond yields during combuſtion a larger quantity of carbonic acid than charcoal. From which it has been concluded that diamond is pure carbonaceous matter, and that charcoal is its oxyde. This is in all probability juſt, but ſtill not perfectly demonſtrated. It is clear that diamond contains more carbon than charcoal, but it is difficult to ſee any ſatisfactory evidence that charcoal is oxyde of diamond. At leaſt it is known that charcoal

contains hydrogen, and a hydruret of diamond would ſhew preciſely the ſame reſults as are actually obſerved, only a ſmall portion of water would be formed in the proceſs, which muſt be too little to attract attention. Diamond detonates with nitre, forming carbonate of pot-aſh, and ſulphuric acid acts upon powder of diamond, being itſelf decompoſed, and a black matter formed of the powder. Morvean ſuppoſes diamond to be pure carbonaceous matter, and plumbago, charcoal, carbonaceous oxyde, and carbonic acid, to be diamond united with ſucceſſive portions of oxygen. Such is at preſent the moſt generally received doctrine of the combinations of the carbonaceous principle, and after all it is certainly the moſt probable account of the matter.

A curious obſervation was made by Mr. Watt. He found that carbon and chalk being expoſed together to conſiderable heat in cloſe veſſels, the chalk became lime and the charcoal diſappeared. But it is known that charcoal alone will bear any heat if air is not admitted. From which it is probable that the carbonic acid of the chalk being forced off by the high temperature diſſolved the charcoal in its paſſage, and formed with it gaſeous oxyde of carbon. Now it is very remarkable that long before this experiment was thought of, a ſimilar one had been made with the ſubſtitution of diamond powder for charcoal, and ſimilar reſults had been obtained. Diamond which bears any heat alone is diſſipated when in the ſame circumſtances it meets with carbonic acid. At leaſt a mixture of chalk or burnt hartſhorn with diamond, being expoſed to heat, the diamond partly diſappears, and partly its ſurface becomes rough, reſembling the diamond when dug out of the earth. It is probable that the ſame quantity of chalk will produce a leſs effect upon diamond in this way than upon charcoal.

The diamond, even of the coarſeſt ſpecies, is ſo expenſive that it is improbable that any general advantage can be derived from our increaſed knowledge of its properties. In a chemical point of view, however, this may not be the caſe. It ſeems likely that

diamond powder may become an uſeful auxiliary in analyſis. At leaſt if diamond and charcoal really differ from each other only by the latter containing oxygen, the diamond muſt attract oxygen with more force than charcoal poſſibly can. Some ſubſtances therefore which refuſe to yield their oxygen to the latter, may obey the influence of the former. And it appears to me that it is worth the attention of thoſe philoſophers, who can afford to make experiments upon this valuable mineral, to try the truth of theſe ſuppoſitions. The boracic and muriatic acids which have hitherto reſiſted all attempts to analyſe them, might be ſubjected to the treatment propoſed. There is, however, one circumſtance which leſſens the probability of ſucceſs by theſe means. This is the extraordinary hardneſs of the diamond, which ſeems to indicate a very great coheſive attraction between the particles of which it is compoſed. Whence, although the diamond muſt attract, or rather retain oxygen with greater force than charcoal, yet as the effect of the attraction exerted by the particles of the diamond for the oxygen muſt be diminiſhed by their attraction for each other, the deſired decompoſition may not enſue. And in general the effect which the particles of two different bodies produce upon each other, muſt be in the direct ratio of the attraction of the heterogeneous particles for each other, and in the inverſe ratio of the attraction of the homogeneous particles for each other. This is probably the reaſon why many bodies differ extremely in their power of retaining other bodies, and their power of attracting them from an exiſting compoſition. It remains to be aſcertained whether the coheſive attraction of the diamond is too ſtrong to allow its other attractions to operate, and whether this coheſion may not be ſo overcome by trituration, or heat, or other means, as to enable the particles to perform the part aſſigned to them.

Almoſt all the experiments which have been hitherto made and the theoretical diſcuſſions regarding them have had reference to the diamond in its colourleſs tranſparent and perfect form. It is, however, frequently found in a ſtate very different from

this. In many inſtances it is coloured by a volatile matter; there are frequently black ſpecks contained in the ſubſtance of the diamond; and the outſide of the cryſtal is always covered in its natural ſtate by a coat of extreme hardneſs, and no luſtre, probably of the nature of the diamond itſelf. We have now a clue which may poſſibly lead us to the determination of the cauſe of theſe different circumſtances.

The colour of diamonds is of various tinges; in one reſpect, however, theſe tinges frequently agree, in volatility. Many of them, when the diamond is expoſed to heat in cloſe veſſels, diſappear. This property does not correſpond with the ſuppoſition of a metallic colouring principle; at leaſt, the metals uſually found to give colour are of a more fixed nature. Beſides the metals do not impart any colour till they are oxydated, and it is difficult to imagine how an oxyde of any kind could exiſt in the ſubſtance of the diamond, which attracts oxygen with ſo much greater force than any metal. The moſt probable ſource of this colour appears to me to be carbon. It is difficult to gueſs how the diamond has been formed by nature, or through what changes or ſituations it may have paſſed. But whether it has been always diamond, or formerly only carbon, it may be conceived to contain a little oxygen united with ſome of its particles, which the application of caloric may cauſe to fly off in the ſtate of carbonaceous oxyd. It is not eaſy to drive all the colour out by heat; the laſt portions probable are leſs oxydated and conſequently leſs volatile. The truth of this hypotheſis might be determined by repeating Lavoiſier's experiment quoted above, but ſubſtituting a very dark coloured diamond in place of a perfect one. If the colouring matter be really an oxyde of diamond, leſs carbonic acid will be produced by the ſame quantity of baſe.

The ſpecks found in the ſubſtance of diamond are of a black appearance. I do not doubt that they too are oxyde of diamond or charcoal, proofs of partial decompoſition or imperfect formation, and of conſequence chiefly in the conſideration of the probable origin and ancient natural hiſtory of this mineral. The cruſt

of the rough diamond is alſo likely to turn out carbonaceous. It is well worth examination, the carbon would perhaps be found united to a portion of the neighbouring ſtratum. The experiments already made on diamonds have ſhewn that an appearance reſembling the cruſt of the rough diamond may be produced on the poliſhed mineral by expoſing it to heat in contact with chalk.

After the conſideration of the facts now diſcovered regarding the diamond, the mind naturally turns itſelf to the poſſibility of forming diamonds artificially. It cannot be ſaid that this diſcovery, if made, would be of any great advantage to mankind. It is difficult to conceive to what uſe this ſubſtance could be generally applied, and though the invention of a method of making diamonds might poſſibly procure a fortune to the inventor, a publication of the proceſs would infallibly leſſen the value of diamonds ſo prodigiouſly that the manufacture would ſcarce be worth attempting. If our theories be right, however, there is no want of materials from which diamonds might be made. All nature abounds with charcoal or its combinations, and whole mountains, nay kingdoms, are compoſed of chalk and limeſtone, in which diamonds ought to be contained in aſtoniſhing abundance. But plentiful as this principle may be, there appears at preſent no likely method of obtaining it pure. The combination of the oxyde of diamond with oxygen itſelf cannot be decompoſed by art, though nature daily effects it in the vegetable economy, nor thoſe immenſe quantities of charcoal ſet free which would contribute ſo materially to the accommodation of the human race. Much leſs then are chemiſts able to decompoſe charcoal itſelf, to which the firſt doſe of oxygen muſt adhere with greater force than any ſucceeding one. Carbon, of all known ſubſtances, attracts oxygen moſt ſtrongly. No ſingle affinity therefore can diſſolve the union of diamond and oxygen. But better hopes may be entertained of double affinities. In this way a ſudden decompoſition of charcoal could only produce ſome very ſmall grains of diamond in the form of fine powder. To make a

cryſtal of that ſubſtance would require either a proceſs advancing very ſlowly, and allowing the particles time to arrange themſelves regularly, or ſome means of reducing the whole to fuſion. Phoſphorus and charcoal, and ſulphur and charcoal diſtilled together, do not leave a reſidue having any reſemblance to the diamond. This ſeems a contradiction to Berthollet's doctrine that a large quantity of a weaker body will always partially decompoſe a union ſupported by ſtronger affinities. On the whole the proceſs of making diamonds does not ſeem at preſent very likely to be put into practice, and it muſt be confeſſed that the whole propoſal bears a very romantic aſpect, and is apt to remind one of the times of alchymy.

The natural hiſtory of this curious mineral is no leſs extraordinary and intereſting than the account of its chemical properties. To geologiſts it ought to afford the moſt important conſiderations, and it is much to be wiſhed that ſome well-informed mineralogiſt ſhould view and report the ſtate of the ſtrata or rocks in the vicinity of which it is found. The nature of the ſtratum in which the diamond itſelf lies it is particularly deſirable to know. It appears that the diamonds do not lie in cluſters, at leaſt that many are not found together. They rather ſeem to be thinly ſown over a large ſurface. It is pretty clear, that they have been fluid themſelves, or precipitated from a fluid. Their cryſtalline form ſhews this, as well as the extraneous ſubſtances which they occaſionally inveſt. It is altogether impoſſible to decide whether they have always been in their preſent ſtate as to chemical properties or not. But it ſeems a more probable ſuppoſition that they have been formed from common carbonaceous matter, during ſome of the revolutions and proceſſes which have taken place in the great elaboratory of nature.

ESSAY XIII.*

ON THE QUANTITY OF MATTER IN BODIES.

THAT the quantity of matter in bodies is in proportion to their weight has long been a received opinion. The ſubſtances, which we are commonly acquainted with, are ſo manifeſtly poſſeſſed of weight, that it never was doubted that all bodies were poſſeſſed of it. Among theſe bodies ſome, though of equal bulk, are heavier than others; a circumſtance not difficult to account for, by the particles of matter in the one body being more cloſely preſſed together than they are in the others. As all bodies, when raiſed to a ſmall height above the ſurface of the earth, upon being deprived of ſupport, immediately fall towards it, it was difficult to imagine any ſubſtances deprived of this property: and though ſmoke and vapour riſe upwards, inſtead of falling downwards, it required but a ſmall ſhare of philoſophy to ſee that this

* This eſſay is dated February 12, 1773.

depended upon theſe bodies being lighter, bulk for bulk, than the air, which every where ſurrounds this globe; and that the tendency of ſmoke and vapour upwards, was of the ſame nature with the riſing of a piece of cork or wood to the ſurface after it had been preſſed to the bottom of a veſſel containing water.

That the quantity of matter is in proportion to the weight of a body, is ſo agreeable to the common ſenſe of mankind, that it ſeems ſcarce ever to have been called in queſtion. When natural philoſophy firſt began to be cultivated to advantage, by fact and experiment, inſtead of fancy and hypotheſis, ſtronger proofs of this doctrine were ſoon obtained. It was found that all bodies, how different ſoever in other reſpects they might be, upon being let fall from the ſame height in veſſels where there was no reſiſtance from the air, apparently fell through equal ſpaces in equal times; and that therefore gravity acted upon all bodies in proportion to their quantity of matter, otherwiſe this could never have happened. The chemiſts, in the experiments by which they attempted to inveſtigate the properties which diſtinguiſh bodies from each other, though they frequently found that ſomething had eſcaped them during their operations, and which might eaſily be accounted for from the volatility of ſome part of the body, were often ſurpriſed to find, that the weight of the principles into which the body apparently was reſolved, inſtead of being leſs, was greater than it

was before: and this happened in ſituations where it was difficult to underſtand upon what this increaſe of weight depended. This is in no caſe more remarkable than in various experiments that have been made on metallic ſubſtances. Theſe bodies, it is well known, upon being expoſed to a proper degree of heat, loſe almoſt all their diſtinguiſhing properties; from a hard ſolid ſubſtance they are converted into a heavy powder, which, except in ſpecific gravity, has ſcarce any reſemblance to the body from which it was produced. What has the moſt attracted the attention of the chemiſts in theſe experiments, is the increaſe of weight, which the metallic ſubſtance acquires by this proceſs: an increaſe which, though different in different metals, is, however, conſiderable in all of them, that are capable of being calcined. This increaſe, on many occaſions, is one-tenth, in ſome it is one-eighth or more. In this proceſs the metal does not ſeem to be acquiring any thing from the air, the only body with which it is in contact. On the contrary, it ſeems rather to be loſing ſomething, as appear sprobable from the alteration in its qualities, and the ſmoke which conſtantly ariſes from it during this proceſs. The cauſe of the increaſed weight in theſe experiments has generally been ſuppoſed to be a quantity of igneous particles, which adhere to the metal after its calcination. If this has any meaning at all, it muſt be, that a quantity of the heat of the fire adheres to the metal, and increaſes its weight. But whatever additional heat

may be gained by the body during its calcination, all this muſt have eſcaped from the body when it arrived at the temperature of the air, from its properties with regard to heat. At the ſame time, we have no proof of a body becoming heavier in conſequence of its being made hotter.

After it was diſcovered that fixed air was capable of uniting with bodies, that it increaſed their weight, and remarkably altered their properties, it was imagined, that during the time a metal calcines, it gradually abſorbs a quantity of air, and in conſequence of this becomes heavier. This opinion was rendered probable, by obſerving, that a metal would not calcine if the air was excluded. The violent effervescence which attends the reduction of a metallic calx, was ſuppoſed to depend on the ſeparation of that air which the calx had abſorbed during its calcination. This opinion is ſo probable, and at the ſame time apparently ſo well ſupported by experiment, that about ten years ago I made ſome experiments,* to determine with what degree of force a metallic calx attracted fixed air. I imagined that quick-lime, upon being mixed with the calx of a metal, would become mild, while the calx would loſe the weight which it had gained before in the fire; or, if it produced no effect, the moſt obvious concluſion was, that metallic calces attracted fixed air more ſtrongly than the calcareous earth did; and of conſequence,

* The records of theſe experiments are in my poſſeſſion. E.

were it poſſible to calcine a metal in ſuch a ſituation, that it could get no air, this calx, upon being mixed with chalk, ſhould inſtantly convert it into lime.—Theſe experiments were ſuch as would inſtantly determine the point in queſtion. As it was found that no effect was produced upon the calx of a metal by mixing it with lime-water, it was concluded that the calx of a metal, free of air, would convert chalk into lime. Metals, by ſolution in acids, loſe the ſame principle which they do in the fire; for we find, that by precipitating a metal from its ſolution in any acid by an alkali, it is always in the form of calx. If an alkali, free of air, were made uſe of, the calx ſhould neceſſarily be free of air alſo. Upon preparing a quantity of calx in this manner, it was found to have gained as much as it would have done by being calcined in the fire.

Fixed air evidently then is not the cauſe of the increaſe of weight, which a metal acquires by being calcined. It was not an eaſy matter to diſcover the cauſe of this increaſe of weight, ſince it happened in ſuch different ſituations, that nothing common could be diſcovered. As this increaſe happened equally in the fire and by ſolution in acids; as the ſum of the weight of the acid and metal before the ſolution was leſs than after the ſolution, though a great quantity of acid was manifeſtly loſt; and as this increaſe was ſtill more evident upon weighing the metal after its precipitation from the acid, ſo it appeared probable, notwithſtanding in all theſe experiments ſomething

was ſeparating, that the weight increaſed while the quantity of matter diminiſhed.

Dr. Black, upon conſidering theſe experiments, hazarded an explanation, that involve conſequences which were little imagined when it was firſt propoſed. All the chemiſts agree that there is a principle common to inflammable and metallic bodies. Dr. Black imagined that this principle is poſſeſſed of this ſingular property, that it not only is not ſubject to the power of gravity, but is poſſeſſed of one directly oppoſite to it; ſo that inſtead of increaſing the weight of the body to which it is united, it remarkably diminiſhes it. As this was exactly contrary to the common ideas of mankind concerning matter, the apparent abſurdity of it was attempted to be taken off, by endeavouring to ſhew that the idea did not involve an abſurdity; that it is eaſy to conceive a body with a repulſive power towards the earth, and that this is of the ſame nature with the pole of one magnet repelling that of another; and that beſides, whatever is the cauſe of gravity, is not ſubject to its own effect.

Inflammable ſubſtances, according to this doctrine, ſhould be heavier after their inflammation, or after the ſeparation of the principle on which this depends, than they were before: this is proved by obſerving, that phoſphorus of urine, exactly counterpoiſed in a balance, upon being ſet on fire, burns with great violence, the ſcale on which it is preponderates, and it gains near three drachms and a half in the

ounce, or almoſt one half. Thoſe perſons too, who employ themſelves in burning ſulphur, in order to make the vitriolic acid for the purpoſes of bleaching or dyeing, obtain, independent of the water mixed with the acid, at leaſt twenty ounces from every pound of ſulphur, though a ſuffocating vapour is felt every where in the room, while the ſulphur is burning. Some of the metals, particularly zinc, burn with as much violence as any inflammable ſubſtance, and with an evident ſmoke. By this proceſs a calx remains heavier by one-ſixth than it was before, which diſſolves in acids, and forms with them the ſame compounds as formerly, with no alteration in weight, and with no efferveſcence. Acids, therefore, which efferveſce with zinc in its natural ſtate, ſeparate from it the ſame principle which fire ſeparates; and the ſeparation of this principle is the cauſe of the efferveſcence, and the cauſe too why the vapour which ariſes during the ſolution is inflammable. If a piece of ſilver be put into aquafortis, contained in a veſſel ſo conſtructed, by means of a valve, that any thing may paſs out of the veſſel but nothing can get into it, I find, that as the ſilver diſſolves in the aquafortis, the ſcale in which they are preponderates; though, during the ſolution, an elaſtic vapour came out of the veſſel, which, by its ſmell, could be perceived every where in the room in which the experiment was performed. The increaſe of weight here was not ſo ſmall as to be aſcribed to any fault in the balance, for it was above

thirty grains in the ounce of ſilver; and nothing could have got into the veſſel but what muſt have paſſed through the pores of the glaſs.

When we take the calces of metals, either prepared by fire, or precipitated from their ſolution in acids by cauſtic alkalies, and expoſe them to the ſun's rays, we find that they recover the properties they had loſt, and diſſolve in acids with the ſame appearances as at firſt. When we encloſe them in glaſs bubbles that are hermetically ſealed, we find that they become lighter than they were before.—This is related by Mr. Boyle to have happened in many experiments made, with a view which he does not mention; but who ſurely cannot be ſuppoſed to have been miſled by any partiality for an opinion concerning the quantity of matter in bodies which he did not entertain. If the heat of the fire, or the air in contact with the metal during its calcination, be the cauſe of this increaſe of weight, why does a ſimilar increaſe appear by ſolution in acids, or by precipitation with alkali, where there is no increaſe of heat and no air? Or, if it be part of the acid, the water, or the alkali, that is the cauſe of this, why ſhould the ſame thing happen in the fire where there is neither acid, alkali, nor water near it? Or why, in hermetically ſealed glaſs veſſels, ſhould the rays of the ſun reſtore the metal to its original ſtate, and diminiſh the weight of the body?* The rays of

* It can ſcarcely be neceſſary to remark, that the more recent diſcoveries in chemiſtry afford a ſatisfactory anſwer to theſe queſtions upon different grounds.

light, or ſomething equally ſubtle with the rays of light only could get at the body in this ſituation. That the ſeparation of a principle from bodies increaſes their weight, and the addition of it diminiſhes it, and that weight is not the meaſure of the quantity of matter in bodies, ſeems to be as well eſtabliſhed by experiment, as experiment is capable of eſtabliſhing any thing. At the ſame time, if weight be not the meaſure of the quantity of matter in bodies, or what is the ſame thing, if gravity does not act upon bodies in proportion to their quantity of matter, ſome of the moſt univerſally, and apparently beſt eſtabliſhed properties of bodies can have no foundation in nature. Theſe conſiderations are ſuch that they are ſufficient to throw a doubt upon the whole, and deter one from any further proſecution of a ſubject, where there is no little probability of ſucceſs.

As it is not, however, becauſe an opinion is received, but becauſe it is well founded, that it is embraced by the philoſopher, and as the properties which exiſt in nature will bear the teſt of experiments, however varied, it appeared proper to beſtow a little more attention on the ſubject, and to try by ſome properly directed experiments, whether the weight is really increaſed, though the quantity of matter is diminiſhed. The firſt conſequence from this doctrine would be, that a metal in its perfect ſtate, and in the form of a calx, ſhould not fall through equal ſpaces in equal times. For the metal in its perfect ſtate containing more matter, though it

weighs leſs than the calx, is leſs acted upon by the power of gravity, and of conſequence ſhould deſcend with leſs velocity than the calx, the reſiſtance of the air being taken away or made equal in both caſes. The velocity with which a heavy body deſcends is moſt accurately determined by the motion of a pendulum. Sir Iſaac Newton has demonſtrated, in the 24th propoſition of the 2d book of the Principia, that in pendulums, whoſe centres of oſcillation are equally diſtant from the centres of ſuſpenſion, the quantity of matter in each pendulum is in the compound ratio of their weight, and the duplicate ratio of the times of their vibrations in vacuo. And as a corollary from this, if the times of vibration are equal, the quantities of matter in the pendulum will be as their weight; and if the weights are equal the quantities of matter will be as the ſquares of the times of their vibrations or inverſely as the lengths of the pendulums. By examining the times of vibration of two pendulums of equal lengths, the one made of a metal in its perfect ſtate, the other of the ſame metal, in the form of a calx, we can determine whether gravity acts upon bodies in proportion to their quantity of matter.

Sir Iſaac Newton, ſeems to have been the firſt that found out a method of accurately determining this point, and he tells us in the 6th propoſition of the 3d book, that with experiments made with this view upon pendulums of gold, ſilver, lead, glaſs, common ſalt, wood, water, and wheat, in which

the length, weight, figure, and reſiſtance of the air, were equal, that they all vibrated in equal times, and that by theſe experiments he could have determined an alteration in the quantity of matter within one thouſandth part of the whole. He does not, however, mention the method he took to determine accurately the centre of oſcillation of theſe different pendulums, which is by no means an eaſy matter. A very ſmall inaccuracy, in this reſpect, would have produced very great differences in the reſult of the experiments. While in theſe bodies the difference between the quantity of matter and the proportional weight might be leſs perceptible than in ſome others. For theſe reaſons, it ſeemed proper to repeat theſe experiments with all poſſible attention, and that the centre of oſcillation might perplex as little as poſſible, a pendulum was conſtructed, that whatever its figure or the diſtance of the ball from the centre of ſuſpenſion might be, its centre of oſcillation ſtill remained unaltered, and it ſtill vibrated in the ſame time.

The principle upon which this pendulum was conſtructed was the ſame with that of the parallel rule. Two rods of ſtraight grained wood were ſuſpended at an equal height from the ground; the points of ſuſpenſion were at the diſtance of eighteen inches. Each of theſe rods, at the lower end, had a piece of braſs with a ſlit in it fitted to them, through which an iron bar paſſed and reſted on them, by means of two wedges ſimilar to thoſe which ſup-

port a common balance. The rods were parallel to each other, and of conſequence, their diſtance from each other below was equal to that above, or eighteen inches, and this was the length of the iron bar betwixt the two wedges, on which it reſted. To the middle of this bar was fixed a rod of iron, perpendicular to the bar, and parallel to the rods of the pendulum. Upon this a ball ſimilar to that of a common pendulum was fixed: this was ſix inches in diameter and two inches thick in the middle, and could be moved upwards or downwards upon the iron rod. By experiment, this pendulum vibrated in the ſame times in whatever part of the perpendicular rod the ball was fixed, ſo that the centre of oſcillation could not be altered by moving the ball of this pendulum, as it is in a common one. The ball of this pendulum was compoſed of two lenticular plates joined by their flat edge with ſcrews. This ball contained the ſubſtance, of which the times of vibration were to be examined, that the reſiſtance of the air might be in every caſe the ſame. The pendulum, thus conſtructed, was always of the ſame length, whether its ball were filled with the ſubſtance examined, or only half full, and the reſiſtance from the air equal in all caſes.

This pendulum I intended to apply to a clock, which was ſo made, that the power by which it maintained the vibration of the pendulum was not ſubject to alteration, from any difference in the maintaining power of the clock, or conſiſtence of the

oil. And that no irregularities might happen, from the effect of heat on the rods of the pendulum, I propoſed, by means of a ſtove, to keep the room where the clock ſtood as nearly of the ſame temperature as poſſible. The time kept by the clock was to be examined by the tranſit of a ſtar, through the croſs hairs of a teleſcope fixed firmly in one poſition. If this clock went faſter, when its ball contained red lead, than when it contained lead, then gravity does not act upon bodies in proportion to their quantity of matter. And it is evident by this contrivance, much ſmaller differences in this reſpect might be obſerved, than by any experiment, however carefully made, with common pendulums. I expected before this time, I ſhould have been able to relate the ſucceſs of theſe experiments. But I was diſappointed in the clock. The clockmaker, who got it to make ſome alterations to fit it for theſe experiments, did not return it in ſufficient time. I made a few experiments with the pendulum alone, but have not determined any thing ſatisfactory. By a number of trials, when the ball contained four pounds of lead, it vibrated 172 times in three minutes meaſured by a ſtop watch, or $57\frac{1}{3}$ times in a minute. When the ball contained four pounds of red lead, the difference of the number of vibrations in three minutes was not perceptible. But in a greater number of vibrations, perhaps, it might be more remarkable.

But ſuppoſing the experiments ſhould ſucceed, it might ſtill be urged againſt gravity acting upon bodies in proportion to their quantity of matter, that, though all bodies deſcend through equal ſpaces in equal times, where there is no reſiſtance, this is no proof of their quantity of matter being in proportion to their weight; for the vis inertiæ of a body may alter in the ſame manner as the gravity. For though the calx be heavier, and contains leſs matter than the metal, and falls through equal ſpaces in equal times with it, this may be owing to the vis inertiæ being increaſed in the ſame proportion with the gravity. And the ſame principle which diminiſhes the weight, while it reſtores the calx to the metallic form, may diminiſh the vis inertiæ exactly in the ſame proportion; ſo that the gravity will appear in both caſes to be the meaſure of the quantity of matter, ſo far as this can be determined by the deſcent of heavy bodies, and the motion of pendulums. But if this be the caſe, inactivity is not an infallible characteriſtic of matter, ſince this can be ſuppoſed to be made greater or leſs at pleaſure, and that of itſelf it has a greater diſpoſition to motion at one time than it has at another. This is an opinion, which brings with it very ſerious conſequences, which are left to thoſe who know them, or can explain them better. One, at leaſt, may be mentioned; it ſuppoſes matter capable of ſelf-motion,

It may be attempted to reconcile theſe opinions by ſaying, that there is a fluid in nature which is ſubject to gravitation, and ſo ſubtile, that it pervades the pores of glaſs, or even of the denſeſt bodies; that this fluid unites with metals during their calcination, and with inflammable ſubſtances during their burning, and is the cauſe of the increaſed weight obſerved in theſe experiments; and that the ſame fluid unites with the metal during its ſolution in acids. But whatever degree of probability there may be of the exiſtence of ſuch a fluid from the phenomena of light and electricity, yet the prodigious quantity of it that muſt neceſſarily unite with the metal, in order to produce this increaſe of weight, cannot eaſily be conceived without giving it ſome other properties, which would neceſſarily make it ſhew itſelf in a more remarkable way than it does. And as this fluid muſt exceed even the electrical fluid in ſubtilty, it ſhould not, any more than that, increaſe the weight of thoſe bodies on which it is accumulated. Now the electric fluid, by paſſing in conſiderable quantity through the calces of certain metals, reſtores them to their metallic form, and gives them the power of effervеſcing with acids, which they had not before the ſhock, and at the ſame time diminiſhes their weight, as I found by experiments made on this ſubject many years ago.

NOTE BY THE EDITOR.

It can hardly be an object of attention at this period of chemical knowledge, to detail the cauſe of the phenomena conſidered in this eſſay, according to the explanation afforded by the antiphlogiſtic theory. I need only obſerve, that the diſcovery of oxygen gas immediately led the way to point out the ſource of the increaſed weight in metallic calces and burnt bodies. This eſſay, notwithſtanding, in my opinion at leaſt, ſhews the progreſs of men's minds towards that diſcovery, and affords a good view of the hiſtory of the tranſaction previous to the period of the propoſal of the preſent theory. It is not neceſſary to make any remark on the reaſonings on the quantity of matter in bodies, which are equally juſt now as they ever were, but there are alluſions to experiments performed by Dr. Irvine upon the increaſe of weight of metallic bodies by ſolution in acids, and precipitation by cauſtic alkalies, and alſo upon the increaſe of weight of the whole maſs of acid and metal during ſolution, notwithſtanding the emiſſion of copious vapours. I ſhall now proceed to give a ſketch of ſuch of theſe as I am in poſſeſſion of.

EXPERIMENTS

On the Alteration of Weight of the whole Mass of Metal and Acid, when metallic Substances dissolve in Acids.

In theſe experiments, the metal, acid and veſſels were firſt weighed with accuracy, the metal was then introduced into the acid, and after their mutual action was finiſhed, the proceſs of weighing was repeated, and the loſs or gain eſtimated from the compariſon of the ſum of the weights of metal and acid before ſolution, and the ſum of the weights after ſolution.

1. Two drachms of clean iron wire were diſſolved in weak vitriolic acid; after ſolution and emiſſion of vapours, two grains were found to be loſt of the groſs weight.

2. Two drachms of clean iron wire were diſſolved in weak muriatic acid, and three grains and a half were miſſing.

In both theſe experiments, the heat and emiſſion of vapours were conſiderable. They were both repeated with exactly the ſame ſucceſs.

3. Two drachms of mercury were diſſolved in diluted nitrous acid, and there appeared an increaſe of nearly a grain in the groſs weights.

4. Two drachms of mercury put into three drachms of aquafortis, after their action had ceaſed, appeared to have gained in their joint weights a grain and nearly a half.

5. Two drachms of zinc were diſſolved in two drachms of vitriolic acid, diluted with twice its weight of water, and the compound had gained nearly a grain, though the heat and emiſſion of vapour were conſiderable.

6. Two drachms of zinc were diſſolved in three drachms of aqua fortis, diluted with twice its weight of water, and a grain of the groſs weights was loſt.

7. Two drachms of biſmuth were diſſolved in half an ounce of aquafortis; there was neither gain nor loſs, the balance remaining in the moſt exact equilibrium.

8. Two drachms of biſmuth were diſſolved in half an ounce of aquafortis, diluted with half an ounce of water, and two grains appeared to have been gained.

Experiments were likewiſe made on all the remaining common metallic ſubſtances, except gold, ſilver, and copper.

Several of theſe experiments were repeated by Dr. Black with the ſame ſucceſs. He diſſolved half an ounce of ſilver in an ounce of nitrous acid of the aquafortis ſtrength, and he found it gained ten grains. It is not eaſy to diſſolve tin and antimony in aqua regia, and iron in the nitrous acid ſo ſlowly and with the emiſſion of ſo few fumes as theſe experiments require; lead does not diſſolve readily in a weak nitrous acid without heat.

Though the experiments now ſtated do not by any means prove that the increaſe of weight in metallic oxydes ariſes from the removal of a principle of levity, yet they ſeem to be very

ſingular in their reſults, and to ſhew that the air muſt afford ſome part of its compoſition to the diſſolving materials. This too, muſt be much more conſiderable than appears at firſt ſight, and muſt amount not only to the weight of the few grains gained, but to the weight of all the fumes and vapours beſides. This principle attracted from the air of the atmoſphere, can hardly be any other than oxygen gas. In the caſes of ſolution by nitrous acid, the azote cannot be ſuppoſed to become deficient, and there is no pretence for ſuch a ſuppoſition, when other acids are employed: neither does there appear any reaſon to attribute the increaſe of weight to moiſture abſorbed, ſince theſe ſubſtances do not appear in other circumſtances to be of a deliqueſcent nature, or to have an unuſual attraction for water. The abſorption of oxygen in theſe caſes appears to reſemble the inſtances of affinity which are diſtinguiſhed by the appellation of predifpoſing affinity; for neither the acid nor metal ſeparately expoſed to the air is increaſed in weight. Now this may happen in two ways; either the acid unites with the metal in the ſtate of what Dr. Thomſon calls a protoxyde, and this compound greedily abſorbs oxygen wherever it can meet it, or the nitrous gas in the caſe of nitrous acid, and the hydrogen, when other acids are employed, in their naſcent ſtate, are again attracted by the oxygen of the atmoſphere, and nitrous acid or water formed again. Either of theſe proceſſes taking place would account for an increaſe of the groſs weights notwithſtanding the emiſſion of fumes. In the caſe of nitrous acid perhaps it might be determined to which of theſe the increaſe was owing. For if the acceſs of oxygen were prevented it would be eaſy to obſerve whether the acid would diſſolve the ſame quantity of metal as when expoſed to the action of the air. If it did clearly the oxygen attracted by the nitrous gas could not have been the ſource of the increaſed weight. It is not ſo eaſy to give a rationale of the experiment mentioned in this eſſay, where a bottle was uſed provided with a valve which prevented the acceſs of common air, and notwithſtanding this the weight was very conſiderably augmented. If we could really ſuppoſe the valve to have acted its part with accuracy, and that

no oxygen found its way, the experiment would be one of the moſt ſurpriſing and unaccountable any where related. But one cannot proceed to form theories till ſuch a thing be more fully eſtabliſhed.

I am not exactly aware how far this ſubject has been inveſtigated by actual experiment. But I believe the acetous acid is ſuppoſed to derive aſſiſtance from the atmoſpheric oxygen in the ſolution of copper and lead, as it appears clearly from no ſolution of theſe metals taking place when vinegar is boiled on them, the ſteam preventing the acceſs of air. Such a ſolution, however, immediately commences when the materials are cold, and the air has free admiſſion. I do not know whether it is exactly underſtood that the air influences the common ſolutions of metals in other acids as the experiments above related tend to ſhew. But it certainly is not known that the groſs weights of the ſubſtances employed are augmented in the manner here ſtated. The muriatic acid alſo reſembles the acetous, in refuſing to diſſolve ſome metals when the acceſs of air is prevented.

ESSAY XIV.

ON WATER.

A. D. 1771.

THE curious experiment of Van Helmont, ſince repeated with more accuracy by others, has been conſidered as a proof that water could not only be converted into earths, but into oils, reſins, gums, and ſaline ſubſtances. This, however, muſt not be admitted without conſiderable limitations. That vegetable bodies draw ſcarce any thing from the ſoil in which they grow is confirmed by many experiments, and that ſoils differ principally from each other in the readineſs with which they part with or retain water, and in the eaſineſs or difficulty with which the roots of vegetables pervade them, or are ſupported by them. Van Helmont planted a willow, weighing fifty pounds, in a pot of earth, which he had previouſly dried and weighed, and covered with

a lead cover, ſo that nothing could be admitted to it but water, which he conveyed through a tube fitted on purpoſe in the lead cover, and which was at all times ſhut, except during the watering of the plant. At the end of five years he took off the cover, and ſeparated the plant carefully from the earth. The pot, with the earth, after being well dried, had loſt two ounces of its original weight. The plant weighed 169lbs. 3oz. or had gained 119lbs. 3oz. This increaſe of weight he ſuppoſes was owing to the water having been converted into all the parts of which his willow was compoſed.

Mr. Boyle repeated Van Helmont's experiment; he put the ſeed of a gourd, which does not weigh above a grain, into a pot of earth dried, and prepared as in the former experiment. A plant was produced that weighed 14lbs.; the earth had loſt nothing. This experiment ſeems to prove, in the ſtrongeſt manner, that none of the ſoil in which the vegetables grew had entered into its compoſition. Many experiments of the ſame kind made by M. Bonnet, of Geneva, ſhew, that vegetables will grow not only to a great ſize, but to the utmoſt perfection in the pureſt water. Anemones, tuberoſes, ranunculuſes, grew in diſtilled water to as great perfection, in the variety and beauty of their colours, in the elegance of their ſhape, and fineneſs of their flavour, as they did in the beſt gardens, or in the fields in which they grow naturally. Every body has ſeen the narciſſus, the hyacinth, and other bul-

bous rooted plants growing in water. He even made vines vegetate in water, and found the plant not only grow with ſuch vigour, as to ſend forth ſhoots of ten or fifteen feet long in a year, but it produced moſt excellent grapes. The lemon and the orange tree produced the fineſt lemons and oranges: the liquorice and the gentian, placed in the pureſt water, were ſtill found to grow with vigour, and to have the ſweet or the bitter juices peculiar to them. From theſe experiments it appears, that water is capable of aſſuming a variety of forms. The ſame pure water, merely by paſſing through the veſſels of different vegetables, has been converted into the ſour juice of the lemons, the bitter of the gentian, the ſweet of the liquorice, the fine agreeable flavour of the tuberoſe, and the diſagreeable one of the rue. Beſides being converted into the gummy, the reſinous, the ſaline, or oily parts contained in vegetables, it has been converted into wood, into bark, into leaves, and into flowers. No ſubſtance that we know of is capable of aſſuming ſuch various forms and qualities as the water ſeems to have done in theſe experiments.

Though pure water thus apparently aſſumes the forms of gummy, reſinous, or of oily ſubſtances, we muſt not, with Van Helmont, allow that they are entirely compoſed of it. Experiments indeed ſhew that they contain a great proportion of water; but experiments ſhew likewiſe that they contain ſomething elſe. And though vegetables ſeem, from the experiments that have juſt been mentioned, to

grow in pure water alone, ſomething elſe is neceſſary to their vegetation, without which, they either will not vegetate at all, or if they do, they no longer have that difference in their taſte and ſmell which they naturally poſſeſs; but all of them contain the ſame watery juice very little different in all vegetables.—Every body knows that the great promoters of vegetation are a proper degree of heat and moiſture, and a conſtant admiſſion of freſh air. The ſeed of a plant in the beſt earth, and with plenty of water, will not grow in an exhauſted receiver, whatever degree of heat it may be expoſed to. And Dr. Hales has ſhewn, that air is contained in all vegetable productions, and is conſtantly abſorbed by veſſels deſtined for that purpoſe, and is as neceſſary for their exiſtence as air is to man.

But we find that vegetables, though furniſhed with air, water, and heat, may be placed in ſituations where they either ſcarce grow at all, or grow very weakly, and never arrive at any perfection. A vegetable, in a dark box, conſtructed ſo that the air may eaſily be admitted to it, grows ſlowly, but it never contains any thing but a watery juice. Hence, the rays of light are ſomeway neceſſary, for the perfect growth of vegetables, ſince, when deprived of this, they all agree together in the nature and qualities of the juice which they contain. We no longer find them capable of producing juices of ſuch different qualities: they no longer have that variety in their colour and flavour that they had before. The

moſt pungent vegetables grow inſipid; the higheſt flavoured inodorous, and the moſt variegated in their colours are of an uniform white when excluded from the light. Vegetables which grow in a natural ſituation readily burn when dry. A vegetable bred in a dark box contains nothing inflammable: that ſomething is attracted from the light ſeems therefore probable.

This is not peculiar to them: ſome other ſubſtances, particularly ſolutions of metallic bodies in acids, upon being expoſed to light, attract ſomething from it. I find a quantity of copper, put into the muriatic acid in a dark place, remains perfectly colourleſs, though kept for a year in that ſituation: upon being expoſed to light in a glaſs bubble, hermetically ſealed, it firſt became yellowiſh, then of a dark brown; in a day or two of a beautiful green; while the reſt of the ſolution, kept in the dark, is ſtill as colourleſs as water. This change cannot be accounted for from the air, for the bubble was exhauſted of air: it muſt have proceeded from the abſorption of ſome very ſubtile matter which eaſily pervades the glaſs. This principle, which vegetables attract from the light, mixed with the air they abſorb, and united with the water which they draw from the ſoil, is probably the cauſe of the difference betwixt the juices of vegetables and pure water; and the differences between the juices of vegetables may ariſe from the mixture of theſe ſubſtances with each other in different proportions.

Whether the air which vegetables abſorb is the common air which we breathe, or air which has undergone ſome change by contributing to the inflammation of combuſtible bodies, and the reſpiration of animals, has not been determined by experiment.—An immenſe quantity of the common air is every day altering its qualities, and, by contributing to inflammation and reſpiration, becomes at laſt unfit for theſe, and extinguiſhes flame and ſuffocates animals. In a little time the whole air would be thus changed, were it not reſtored to its original ſtate by ſome conſtant natural proceſs. I imagine the vegetable kingdom performs this operation, though I know no experiment to prove it, nor is it eaſy to contrive one that would be concluſive. We have thus, by examining in a ſlight manner the proceſs of vegetation, found that the opinion, of all the juices in vegetables being pure water, is without ſufficient foundation, though water is the principal ingredient.

The waters of ſome ſprings are remarkable for depoſiting great quantities of earth on the bodies which they touch, and by which they are, in time, converted into ſtone. Theſe petrifying waters have always been regarded as curioſities: but the earth which they depoſit is not owing to the water being converted into earth, but to a quantity of earth which they contain in a diſſolved ſtate. Theſe petrifying waters are not pure, but leave the earth which they contain behind them upon being diſtilled. The water that has the property of converting the ſub-

ſtances on which it falls into ſtone, or which by conſtantly dropping from the tops of caverns, forms a number of pillars or ſtalactites, like icicles from a houſe in froſty weather, differ from one another in the quantity of earth which they contain. The ſpring in the wood at Hamilton, which petrifies the moſs over which it runs, and one at Woodſide, near Glaſgow, contain but little earth. The one at Knareſborough, in Yorkſhire, contains a great deal, ſo that it will harden a vegetable as if it were frozen in 24 hours, and will completely petrify it in two months. This is the moſt remarkable water for this property that is known: it runs over a rock about eighteen feet high, in a great number of ſmall ſtreams, which, falling on moſs at the foot of it, have converted it all into ſtone. It has afforded matter of ſpeculation to the chemiſts, to find out by what means the water has diſſolved this earth. The petrifactions of it, it is well known, all burn into lime. But the earth, in the Knareſborough water, cannot be in the form of lime, otherwiſe it ſhould gather cruſts in the air like lime-water, which I found by experiment it did not. We can ſcarce imagine that it is diſſolved by a ſaline ſubſtance, becauſe nothing of this kind is to be found in the petrifaction, which ſhould, if this were the caſe, be the calcareous earth diſſolved by ſome ſalt, and not a pure earth.

The reaſon why it has been commonly ſuppoſed that it was owing to a ſalt, is from the taſte of the water, which is manifeſtly ſour, with ſomething of

an aſtringent taſte, not unlike a weak ſolution of alum in water. Upon expoſing a little of the water in a ſaucer, no ſcum or cruſt gathered upon it; but as the water ſlowly evaporated, a whitiſh powder fell to the bottom, which would not again diſſolve in the water. A perfectly cauſtic alkali threw down a white powder, which neither would diſſolve again; another proof, that the calcareous earth was not in this water in the form of lime. This experiment ſeemed to prove that it was a ſalt that kept the earth diſſolved; and yet no ſalt being found in the petrifaction, ſeemed to contradict this.

Air that has paſſed through the fire, or been breathed by animals, or which is ſeparated from calcareous earth or alkalis, during their ſolution in acids, readily is abſorbed by water. The water thus ſaturated with air has the exact taſte of Knareſborough water, only a little ſtronger. It is very ſour, changes the blue and purple vegetable infuſions to red, like the acids, and exactly reſembles the water of Pyrmont, which ſeems to contain this air only, or this and a little iron. An experiment made with this water, ſaturated with air, by Mr. Cavendiſh, mentioned in the Philoſophical Tranſactions, ſuggeſted an explanation of the Knareſborough water. This water, mixed with lime water, throws down the lime by furniſhing air to it, and reducing it to its natural inſoluble ſtate: but two parts of water, and one of lime water, upon being mixed at firſt are milky, but the tranſparency is recovered by ſhaking. This experiment

is not eaſily explained, ſo as to agree with the opinion of the calcareous earth being ſoluble in water, in conſequence of its being deprived of air; for the lime had attracted the air at firſt from the water, as is evident by its becoming milky; yet it was even in this ſtate inſtantly rediſſolved.

I made ſome experiments upon this mixture; and the only probable account I could fall upon was, by ſuppoſing that the calcareous earth was not only ſoluble in water, when deprived of air, but alſo when it contained too much of it: and that it was leaſt ſoluble in an intermediate ſtate. This mixture of lime-water with water ſaturated with air, agrees almoſt exactly in all its properties with the Knareſborough water. That the calcareous earth is diſſolved in the mixture of lime-water and water ſaturated with air ſeems to be proved by the following experiment: a cauſtic alkali, which did not at all affect lime-water, turned this muddy, and the calcareous earth ſoon fell to the bottom. Upon examining it, I found that it agreed in every thing with chalk, and like it effervesced with acids. The earth was, in this caſe, precipitated by the cauſtic alkali attracting the redundant air, and reducing it to the middle ſtate in which it is leaſt ſoluble. For this the alkali has a ſtronger attraction than the calcareous earth; but the calcareous earth has a ſtronger attraction for the remaining air, as is evident from Dr. Black's experiments. A cauſtic volatile alkali, which has ſtill leſs attrac-

tion for fixed air than the fixed alkalis, has the ſame effect, and for the ſame reaſon.

If this explanation were juſt, two conſequences neceſſarily flowed from it: 1, That an alkali, already ſaturated with air, ſhould have no effect upon the mixture of lime-water and water ſaturated with air, becauſe it could not attract this redundant air from the calcareous earth, ſo as to reduce it into its middle inſoluble ſtate: 2, That chalk ſhould diſſolve in water ſaturated with air. Theſe were found to be true upon trying the experiments. Common alkalis will not anſwer, as they are far from ſaturation; they muſt be quite ſaturated, and when in this ſtate, the tranſparency of the liquor is not in the leaſt affected by them. This may, therefore, be uſed as a teſt for a mild alkali, as lime-water is uſed as a teſt for a cauſtic alkali. The mixture of chalk and water ſaturated with fixed air is exactly like the Knareſborough water.

The water of the Knareſborough ſpring ſeems to be produced thus. A quantity of air of this kind is found in caverns under ground as in coal-pits, which cannot be wrought, on account of its extinguiſhing the candles of the workmen, and being totally unfit for their reſpiration. If this air is in contact with the water of any ſpring, a part of it will immediately be abſorbed; the water will acquire a ſour taſte, and will ſparkle in the glaſs like the Pyrmont water. If this water, in its courſe, meets with a quantity of

lime-ſtone or chalk, it will diſſolve a ſmall quantity of them, and form a petrifying water, in every reſpect like the Knareſborough water.

The water of all ſprings is impure, compared with rain or ſnow-water: they all contain ſomething of a ſaline ingredient. As rivers ariſe from ſprings, and flow over ſubſtances ſoluble in water, they likewiſe contain ſaline matter, beſides a quantity of putrifying, animal, and vegetable bodies, which they meet in their courſe, and a part of which they diſſolve.

The ſaline ſubſtances which is found in the earth, in the greateſt quantity, is common ſalt. Immenſe beds of it are found in different parts of the world: chains of mountains, even ſome iſlands, are almoſt entirely compoſed of it. No ſubſtance is on this account ſo common in water as ſea-ſalt; no water is entirely free from it. Water is a very volatile body; a very moderate degree of heat is ſufficient for converting it into vapour; even in the ſolid form of ice it evaporates, though more ſlowly than in a fluid form. The ſaline ſubſtances cannot be converted into vapour by any degree of heat which naturally happens in any of the climates of this globe. The water in rivers is therefore conſtantly evaporating during their courſe, while the ſaline parts remain. The proportion of the ſaline parts to the water will therefore be conſtantly increaſing, and will be greateſt in the loweſt part of the globe. Hence, the water of the ocean contains more ſalt than any other water; and thoſe parts of the ocean that are expoſed

to the greateſt heat will be ſalter than the reſt. The ſea water, under the equator, contains more ſalt than in higher latitudes.

The water which is converted into vapour is free of every thing ſaline; and by being driven about by the wind, and attracted by the high mountains, falls down in the form of rain, dew, hail, and ſnow, which ſupply the fountains, and theſe the rivers, which again fall into the ſea; ſo that the water is ſubject to a perpetual circulation, by which it affords new ſupport to the vegetable kingdom, exhauſted by the evaporation of its watery parts. In conſequence of this conſtant evaporation, the freſher water towards the poles muſt tend in a conſtant current towards the equator, otherwiſe the water at the equator would, by this time, have been entirely evaporated, and the whole converted into ſolid ſalt. That there is a current of this kind, is confirmed by the accounts of navigators. As the evaporation goes on principally at the ſurface, the water at the ſurface will ſoon become ſalter than the reſt; but at the ſame time it becomes heavier, ſo that it falls in a conſtant ſtream from the top towards the bottom. Experiment confirms this, for the ſea is ſalter at the bottom than at the ſurface.

If a large bay is formed by the ſea, which communicates with the reſt of the ocean by a narrow neck, the current produced by the evaporation will be eaſily perceived. The Mediterranean is of this kind, which, extending to a ſurface of 300,000 ſquare miles, com-

municates with the Atlantic at the Straights, by a neck about five Engliſh miles over. Through this there is a conſtant current, it is affirmed, of two miles in the hour. This Dr. Halley long ago accounted for from the evaporation from the ſurface of the Mediterranean being greater than what it receives from all the rivers which fall into it, many of which are very large: but if this ſea had loſt annually, ſince its exiſtence, the quantity of water which Dr. Halley ſuppoſes, it would long, ere now, have been converted into a vaſt maſs of indurated ſalt. By calculations from the quantity of ſalt contained in the water of the Mediterranean, it appears, that in 500 years, the ſalt ſeparated would have been 250 feet high. Now the Mediterranean is, in many places, not of this depth. So that this ſea would have been wholly changed into ſalt, if the ſalt-water continually flowing into it had no iſſue: but in all the time it has been known it has not become ſalter.

The explanation ſeems to depend on the water in the Mediterranean being ſalter than that in the Atlantic. By experiment, it is ſaid the former contains near $\frac{1}{16}$, the latter contains $\frac{1}{26}$ part of ſalt. The ſurface of the two ſeas, therefore, being of equal height, their gravity would not be equal; ſo that the two ſeas would run together, the lighter ſpreading on the ſurface, the heavier ſpreading on the bottom. Hence there would be two currents, one from the Atlantic to the Mediterranean on the ſurface; another from the Mediterranean to the Atlantic, on the bot-

tom, in the ſame manner as in a warm room while the cold air ruſhes in at the bottom, the warm air flows out at the top of the door.

NOTE BY THE EDITOR.

THIS eſſay was publicly read in the year 1771 by its author, and ever afterwards formed a part of his courſe of chemical lectures. The records of the ſociety to which it was communicated, and the recollection of many of its members of unblemiſhed integrity and enlightened minds would readily, if neceſſary, give the moſt preciſe evidence of the correctneſs of the date, which is moreover affixed to the manuſcript by the author's hand. This preciſion is requiſite in this inſtance to ſhew that the obſervations contained in this eſſay on the ſolubility of chalk in water, impregnated with carbonic acid, were made without any knowledge of Bergman's experiments on the ſame ſubject, which did not take place, or at leaſt were not publiſhed, till the year 1774. It is certainly very much to be regretted that my father was ſo extremely averſe to commit his obſervations to the preſs, and it has been peculiarly prejudicial to his reputation as a chemiſt. It aroſe, however, I believe in a great meaſure from a nicety, perhaps a faſtidiouſneſs of taſte, which rendered him always diſſatisfied with the degree of perfection to which he had brought his ſpeculations, and partly alſo from an opinion not very well founded that teaching his theories and experiments to his pupils was equivalent to a more formal publication of them.

It will not be here neceſſary to revert to the conſideration of Van Helmont's and Mr. Boyle's experiments on the growth of plants in water, which have been noticed in a former note, nor to the purification of vitiated air by vegetables, and the decom-

poſition of carbonic acid gas, which I have already ſtated Dr. Irvine thought himſelf to have firſt ſuggeſted.

There is an experiment mentioned in this eſſay, however, which deſerves more attention. I allude to the ſolution of copper, made in the muriatic acid, which was colourleſs as long as kept in the dark, but by expoſure to light became firſt yellow, then brown, and finally green. It is a curious inquiry here, how did the influence of light produce this change of colour? There are two muriates of copper, the green muriate which contains a large quantity of oxygen and little copper, and the colourleſs muriate, which contain leſs oxygen and more copper. This latter may, according to Prouſt, be obtained by plunging a plate of copper in muriatic acid, and keeping it for ſome time in that ſituation. It appears then that Dr. Irvine formed this ſalt, which when expoſed to the air ſpeedily attracts oxygen, and is converted into the green muriate. Pains, however, ſeem to have been taken to guard the ſolution from the contact of air. If the effect thus be produced by oxygen, and the oxygen was not derived from the atmoſphere, it muſt have come from the water, and that by the influence of light in ſome manner. The muriate of copper may not attract the oxygen of the water ſo ſtrongly as the hydrogen does; but the light having acceſs may attract the hydrogen with a certain force, which may ſo weaken its affinity for the oxygen that the water may be decompoſed, and the muriate become green. But if this be true, what becomes of the hydrogen? It may be anſwered, that it may aſſume the gaſeous form; or if the recent diſcovery of the compoſition of muriatic acid ſhould be confirmed, the hydrogen may be ſuppoſed to be altered in another way, and combine with the ſolution in the form of muriatic acid. It ſeems not improbable, from the various changes of colour of the ſolution, that copper is capable of combining with oxygen in more than two or three proportions.

I find many records of experiments upon the water of different ſprings and wells by Dr. Irvine; in particular the water of Glaſgow, where he reſided, attracted his attention, and I ſubjoin a

table in which are contained the results of many trials made to determine the comparative purity of water from various wells, rivers, and springs, there and in other places. A bubble was fitted to diftilled water at 56°, and the temperature was noted at which the fame bubble ſunk in various kinds of leſs pure water.

	Degrees	
1	Distilled water	56
2	Clyde	58
3		62½
4		74
5		73½ Temperature of the well, July 29, 1776 = 52°
6		83
7		64
8		66
9		67
10		62½
11		64½ Temp. of the water in the well = 52°, 1776.
12		58 Temp. ditto = 52°, 1776 = a gallon of Arn's well water, left a residuum of only two grains upon distillation.
13		68 Ditto ditto = 52.
14		68
15		62 Temp. July = 50.
16		70
17		65
18		59
19		59 Temp. in well = 46°, Feb. 1780.
20		60½ Ditto = 44°.
21		62½ Temp. 50°, March 1780.
22		64 Temp. 50°. This water contains iron, fixed air, and a little muriatic acid.
23		72½ Contains iron.
24		69½
25		78
26		64½
27		72½
28		73
29		74
30		70
31		64½
32		64½
33		70
34		63½
35		62
36		68½
37		61½ Contains iron.
38		59½
39		63
40		62½
41		70
42		78
43		60½
44		59½
45		59½
46		94
47		169

3. Deanside well water. 4. Well in High-street, at the old venal. 5. Ditto in Mr. Thompson's close. 6. Old well in Par-ames close, new opened and ordered to be shut. 7. Bell's wynd

This table affords pretty fair grounds to determine the degrees of impurity contained in any of the waters examined, but not of the kind, unleſs where particularly ſpecified. The beſt water is the lighteſt, and conſequently the bubble that ſinks in the pureſt water will ſwim in more contaminated ſorts. Therefore, the lower the temperature at which the bubble ſunk in any of the experiments above ſtated, the purer is the water. The uſe of applying heat was to render the water, bulk for bulk, lighter than it otherwiſe was, ſo that it might fit the bubble. The number of degrees in the above table then may be conſidered as indicating the purity or impurity of the different waters, the ſmalleſt number making the leaſt degree of contamination.

This eſſay is not complete. I have abridged its length conſiderably, by omitting a pretty long diſcuſſion on the appearance of earth in frequently diſtilled water, the ſource of which is now better underſtood.

---

ditto. 8. Weſtport well. 9. Weſtern ſugar-houſe cloſe ditto. 10. Incle factory well. 11. Ram's horn well. 12. Arn's well. 13. Salt market well. 14. Well in Dowan's cloſe, Gallowgate. 15. Well of G. Crawford, Eſq. 16. Well in Salt market below Bridgegate. 17. Mr. W. Cunningham's well, Queen-ſtreet. 18. Ditto from Lenſhaw. 19. St. Thomas' well, Poſſil. 20. St. John's ditto. 21. Lady well. 22. Phyſic well. 23. Spoutmouth well. 24. Well oppoſite the guard. 25. Tron gate well, oppoſite the Exchange. 26. Mr. Shortridge's well, Dunlop-ſtreet. 27. Blue pump, Argyle-ſtreet. 28. Red pump, near the head of St. Enoch's Wynd. 29. Hornscourt. 30. Well in Mr. P. Colquhoun's cloſe. 31. Well fronting Buchanan-ſtreet. 32. Well in Mr. Al. Gordon's cloſe. 33. Adam's-court. 34. Well oppoſite Jamaica-ſtreet. 35. Mr. Young's, the bleacher, in Buchanan's crooked wynd. 36. Mr. A. Buchanan's, Jamaica-ſtreet. 37. Enoch's bank. 38. Spring-bank. 39. Ditto well. 40. Two wells at Smithfield. 41. Water from the town hoſpital well. 42. Stock well, Oſwalds cloſe. 43. Gourock Bore well. 44. Greenock ciſtern. 45. Port Glaſgow ditto. 46. Iron ſpring from Sanquhuar. 47. Sea water at Fairlie, in the Frith of Clyde, in dry weather.

PART THE THIRD;

CONTAINING

EXPERIMENTAL ESSAYS.

BY

WILLIAM IRVINE, M. D.

"Et quorum pars magna fui."

preferred for this purpoſe the caſe of ice and water, the moſt familiar in theſe latitudes to the obſervation of men, and infinitely the moſt important of all, from the great ſcale upon which it is effected by nature. It is far from my intention to detail here all the experiments which Dr. Black made to determine this point. One inſtance will be ſufficient. If there be mixed in a proper veſſel a pound of ice at 32° with a pound of water at 172°, the whole becomes inſtantly fluid, and the temperature is 32°. Hence the water was cooled 140°, and the ice not heated at all, but only melted. One hundred and forty degrees of heat, therefore, have entered into the ice during its fuſion, and are neceſſary for producing that effect. That this is really the caſe appears plainly from repeating this experiment with a little alteration. If, inſtead of the pound of ice at 32°, we ſubſtitute a pound of water at 32° the temperature of the mixture is not 32° but 102°, the ſurplus of heat above 32° being ſhared equally by the two portions of water.

Having proved in this manner that ice does not refuſe to admit heat, while fuſing, but that 140° are required to convert it into water, Dr. Black proceeded no further by experiment. He extended his theory, however, to all caſes of fuſion whatever. The 140° which are thus demonſtrated to exiſt latent in water, are meaſured by the capacity or ſpecific heat of water, as is evident from the conſideration of Dr. Black's experiments. For, in order to fuſe

the ice, an equal weight of water is deprived of 140° of heat, which are only ſufficient to ſatisfy the demands of the new capacity. If, therefore, we ſuppoſe the capacity of ice for heat to be as nine, that of water being ten, and nobody has made the difference leſs, the latent heat of water, expreſſed as it is uſual and convenient to do in all other caſes, where it is extremely difficult, if not altogether impoſſible to aſcertain the ſpecific heat of the fluid in queſtion, would be 155°.555, and this mode of expreſſion would undoubtedly be more convenient than the preſent one, where the compariſon of the latent heat of various bodies is to be made, if th capacity of ice were accurately known. Even with our preſent imperfect knowledge of that circumſtance, I ſhall, in this eſſay, ſtate the latent heat of water at 155.555 degrees meaſured by the capacity of ice.

It cannot afford juſt ground for wonder that there ſhould ſtill remain ſome doubt of the accurate ſpecific caloric of ice, conſidering the extreme nicety of all experiments upon heat, not one of which, I may boldly affirm, can aſſert any claim to perfect preciſion. Many other philoſophers have alſo repeated and modified the experiments of Dr. Black, to determine the latent heat of water. And though the caſe is as ſimple and favourable as any caſe can be, the reſults have by no means exactly coincided with each other. To illuſtrate the degree of uncertainty of ſuch determinations, it may be worth while

to compare the numbers found by Black, Wilcke, and Laplace, three of the moſt diſtinguiſhed philoſophers who have turned their attention to this ſubject. It is ſtated in the *Annales de Chimie*, vol. 3, page 171, that the latent heat of water, as determined by Dr. Black, is, in degrees of Reaumur, 62°.22, as found by Wilcke 57°.73, and by Laplace 60°, which numbers may be roundly given, according to Fahrenheit's ſcale, as 140, 130, and 135. Mr. Nicholſon, in his Treatiſe on Chemiſtry, ſtates the latent heat of water at a number conſiderably higher than any of theſe, 146°; but I have not been able to find upon what authority, though I have no doubt of that gentleman's accuracy. Upon the whole there ſeems fully as much reaſon to truſt to Dr. Black's determination of this point, as to thoſe of the other philoſophers; and the numbers, in general, do not differ more from each other than it is reaſonable to expect they ſhould, in all caſes of ſo much delicacy. For it never ceaſes to be neceſſary to recall to the recollection of the reaſoner upon the theories of caloric, that all the numbers expreſſive of the capacities and latent heats of bodies are only approximations to truth, from which they even ſometimes widely diverge.

The only other philoſopher, as far as I know, who has attended to the preciſe quantity of the latent heats of different ſubſtances, is Dr. Irvine. Landriani, it is true, made ſome experiments to prove that the fluidity of alum, ſulphur, and ſome metals was

accompanied with an abſorption of latent heat; but I believe he made no attempts to aſcertain the preciſe quantity. Dr. Irvine inveſtigated this ſubject ſoon after Dr. Black, and the ſubſtances ſubmitted by him to examination were ſpermaceti, bees' wax, and tin. He found the latent heats of theſe bodies to be as follows:

Spermaceti	145°
Bees' wax	175°
Tin	500°

From the imperfect notes which I poſſeſs, of the methods uſed to determine the two firſt of theſe, the 145° are meaſured by the capacity of melted ſpermaceti, and the 175° by the capacity of melted bees' wax. I am perfectly ignorant of every circumſtance regarding the latent heat of tin, as determined by my father, excepting the number itſelf, as I have been unable to find any notes of his experiments for aſcertaining this point; but it is reaſonable to conclude that theſe 500° are meaſured by the capacity of ſolid tin, becauſe it ſeems an attempt of great difficulty, if not altogether impoſſible, to find the capacity of that body in its fluid form. The probability of this is conſiderably augmented by the reſult of ſome experiments which I lately undertook to determine again the latent heat of tin, and in which the numbers agreed very nearly with that here ſtated, on the ſuppoſition that the degrees were intended to be applied to tin in its ſolid ſtate.

It occurred to me, ſome time ago, that it might be poſſible to add a little to the information at preſent poſſeſſed concerning the quantity of latent heat neceſſary for the fuſion of various bodies. Whatever opinion may be entertained of the nature of latent heat, or of the manner in which it enters into bodies, there can be no doubt of its exiſtence in many caſes. It appears therefore, an object of ſome curioſity to compare its quantity in various inſtances, and to trace, if poſſible, any principle by which its entrance into fuſing ſubſtances may ſeem to be governed. But with our preſent ſcanty knowledge of this ſubject, ſuch an attempt is not likely to prove ſucceſsful. There are only four inſtances in which the latent heat has been attempted to be aſcertained, and though the ſubject is ſurrounded by conſiderable obſtacles, yet theſe do not always appear to be of an inſurmountable nature. Influenced by theſe conſiderations, I reſolved to endeavour to extend our knowledge a little upon this ſubject, and, in conſequence, made a great number of experiments, of which I now proceed to give ſome account,

The veſſels which I employed were extremely ſimple, and conſiſted of nothing more than common Florence flaſks of which the necks were cut off at a convenient diſtance. In theſe the water made uſe of was contained, and the veſſel was ſupported on a ſlight wooden ſtand, which preſented a very ſmall ſurface to abſtract heat from the materials. The orifice at the ſuperior part of the veſſel, was, in general, not

more than ſufficient to permit the ready introduction of the fluid examined; probably from an inch and a quarter in diameter to a little more, ſo that a very ſmall ſurface of the water was expoſed to the air. The weight of the glaſs was, in all caſes, accurately aſcertained. Theſe circumſtances being premiſed, I proceeded in the following manner.

The firſt ſubſtance which I ſubmitted to examination was biſmuth. The melting point of this metal was, by the thermometer which I uſed, 480°. This is ſo very near the point found by my father, to wit, 476, that I ſhall conſider his determination as the true one, as he combined and compared the different ways of computation and obſervation, and I know the thermometer which he employed to have been made with particular care. Into a glaſs veſſel which weighed 411 grains, and which was conſtructed and ſituated as above mentioned, I put 2236 grains of water, of the temperature of 62°, which was that alſo of the apartment in which the experiment was conducted. I then removed from the fire a quantity of fluid biſmuth in a crucible; I waited till it was partially ſolid, and the ſolid part projecting into the fluid; at that moment I inferred that both the ſolid and fluid metal were of the common temperature of 476°. This, I believe, is a fair induction. It is impoſſible to ſpare time during the experiment, to aſcertain the temperature of the ſemiliquid. But the melting point of any ſubſtance can be aſcertained in no other way than on

the ſuppoſition that a ſolid immerſed in its relative fluid is either melted by it, or if it remains in the fluid unmelted, and the fluid is not frozen, that both are at one temperature, and that temperature is the melting point. A thermometer introduced into a crucible filled with a mingled maſs of ſolid and fluid biſmuth always points to 476° or very near it. I proceeded to pour a portion of the fluid biſmuth into the veſſel of water. The mixture was well ſtirred, and in one minute the thermometer ſtood in it at 86 ; in two minutes at 85°.75. The true temperature at the moment of mixture was, therefore, 86°.25, ſuppoſing equal temperatures to be loſt in equal intervals. There was alſo a ſmall quantity of ſteam formed. I now weighed the veſſel with its contents of water and biſmuth, and found that it had gained 1555 grains. This, therefore, ought to have been the quantity of biſmuth poured in, if there had been no loſs. But upon drying and weighing the metal, it appeared to amount to 1589 grains, and 34 grains of water or metal have conſequently diſappeared. The biſmuth was cooled 389°.75; the 2236 grains of water were heated 24°.25. Then 1559 grains of water, or a quantity equal to that of the biſmuth, would have been heated more in the proportion of 1559 to 2236, or 34°.77. Theſe 34°.77, in order to be compared with the degrees which the biſmuth has loſt muſt be reduced to the ſame value, or that quantity of heat which they contain muſt be expreſſed by degrees meaſured

by the capacity of biſmuth. This will not alter the quantity of heat, but only give us a different expreſſion of it. The ſpecific heat of biſmuth is ſtated in Thomſon's Syſtem of Chemiſtry at .043: the quantity of caloric which raiſes the temperature of water 34°.77, would therefore raiſe that of an equal quantity of biſmuth 850°.46. But the biſmuth after becoming ſolid, could only have loſt 389.75, which being ſubtracted from 850°.46 there remains 460°.71, which cannot be accounted for by the cooling of the ſolid biſmuth, and which muſt conſequently be the whole or a part of the latent heat of the fuſed metal.*

But the latent heat muſt be greater than this; for 411 grains of glaſs were alſo heated 24°.25. If the capacity of this be taken at .174, as Mr. Kirwan found flint glaſs to have, and I have found green bottle glaſs to have a capacity .173 by ſeveral experiments, whence it is probable that the glaſs of Florence flaſks has its capacity not very wide of theſe numbers; if then .174 be taken as the capacity of this glaſs, theſe 24°.25 which the biſmuth communicated to the 411 grains of glaſs in the veſſel, when meaſured by the capacity of the biſmuth, are equal to 98°.1: and 1559 grains of glaſs would have gained 25°.8, which muſt be added to 460°.71

* Some errors, which however were merely of an arithmetical kind, will be obſerved to have eſcaped me in the hurry of writing this train of reaſoning when it appeared formerly. Theſe are now corrected.

already found, and make 486°.51 for the latent heat of biſmuth.

But this is obviouſly ſtill too little; for, as has been already mentioned, there was a good deal of ſteam formed. The amount of the heat thus loſt, it is extremely difficult to aſſign. I ſhall, however, make an attempt to gueſs at, rather than determine it. All the 34 grains muſt not be reckoned to have been loſt in evaporation. In ſpite of all my efforts I could perceive that ſome, though certainly a ſmall quantity of biſmuth, went off with the water, in the form of a number of very minute particles floating in the liquid: and, perhaps alſo, a ſmall portion of water might be waſted during the proceſs. If we allow 18 grains of the loſs to be accounted for in theſe ways, there would remain 16 grains of water which have been converted into ſteam. The latent heat of ſteam was computed, by Mr. Watt, to be equal to 940°. But this ſteam cannot be allowed ſo much latent heat as this; and though it may be difficult to point out the quantity to be fairly granted, yet I ſhall expect to be within bounds, when I eſtimate it at one-half of 940°, or 470°. In that caſe we ſhould ſay, ſixteen grains of water have been heated 470°; ſixteen grains of biſmuth would be heated by the ſame quantity of caloric, 10930°,23, and 1559 grains + 16 grains, ſuppoſed to have been poured off with the water, = 1575 grains of biſmuth would be heated 112°.43. This quantity of heat, therefore, ought to be added to the

486°.51, already found, and would amount in all to 598°.94 which ſhould be the whole latent heat of biſmuth.

I am perfectly ſenſible that there are ſeveral gratuitous ſuppoſitions in this laſt part of the reaſoning, where it is attempted to eſtimate the amount of the ſteam produced, and of its latent heat; I do not, therefore, lay much ſtreſs upon it. The number 486°.51 is not at all affected by the truth or falſehood of the ſubſequent inductions, and, as far as I ſee, ſtands upon firm grounds. I endeavoured, however, to confirm or refute the truth of the laſt part of my argument by making an experiment exactly on the ſame principles, but where by dexterity, I might prevent the formation of ſteam wholly or in great part. In one inſtance I ſucceeded tolerably well, and then the latent heat, with every correction, amounted to nearly 600°, approaching thus extremely cloſely to the determination by computation.

Having mentioned the word dexterity, it is proper that I ſhould give ſome explanation of what is meant by it here. Though it may ſeem, at firſt ſight an exceſſively eaſy thing to pour fluid biſmuth into water, yet it will be found, upon trial, to be a matter of conſiderable difficulty to do it in ſuch a manner as to make the calculations agree in their reſults. This difficulty ariſes from the unequal quantity of ſteam formed by different management, and alſo from the water ſometimes violently thrown

about by the metal, at the moment of its firſt contact with it. Sometimes a violent exploſion takes place, occaſioned, as I ſuppoſe, by a hollow ſphere of biſmuth being rapidly formed, incloſing a portion of water, which it ſuddenly rarefies, and expands with great force. I cannot, in all caſes give directions how to avoid theſe accidents, but I know very well that they rarely or never happened during the latter part of my experiments. Some obſervations, however, ſuggeſted themſelves to me. Care muſt be taken that the metal is not too near complete fixation, and that no portion of the ſolid metal be poured along with the fluid. Similar cautions muſt be obſerved with reſpect to the oxyde which is always found floating on the ſurface. The method of pouring out the metal alſo requires attention: if it be poured ſuddenly, or with a jerk, a large jet of ſteam is immediately thrown out as the metal touches the water: if even a ſmall ſtream be poured in at one point only, the ſolid metal forms a column in the water which quickly reaches the ſurface, and at that moment an immenſe quantity of ſteam is formed. On the other hand, if the fluid metal be poured from too great a height, it may become ſolid before reaching the water, and thus defeat the intention of the experiment. A little practice, however, is ſufficient to enable any one to avoid or leſſen theſe ſources of inaccuracy.

I repeated theſe experiments for determining the

latent heat of bismuth many times, and the result is expressed in the following

TABLE.

No. of Experiments.	Latent heat by the first computation.	Correction for the heat received by the glass	Whole latent heat.
1	457	23	480
2	411	29	440
3	412	28	440
4	465	33	498
5	480	29	509
6	438	27	465
7	465	30	495
8	460	25	485
Mean	448		476

On the whole, therefore, it appears that we shall not exceed the truth when we estimate the latent heat of bismuth at 550°.

I made, precisely in the same manner, two experiments to ascertain the latent heat of tin, and of these the results were—

Experiment.	Latent Heat.
1	520
2	495
Mean	507

This agrees remarkably well with the determination of the same point made by Dr. Irvine.

Zinc requires for its fuſion a higher temperature that that at which mercury boils. The mercurial thermometer, therefore, cannot be uſed to determine the melting point of that metal. But it was computed by Bergman to melt at 700° of Fahrenheit's ſcale. Taking that for granted, I made three experiments on the latent heat of zinc, in a ſimilar way with that already related with biſmuth. Theſe experiments might be eaſily detailed at full length, but it is to be feared that ſo tedious and minute an account might have a greater tendency to exhauſt the patience, than to add to the information, of the reader. I ſhall therefore only give the reſults of the calculations inſtituted, which I have in this, and every other inſtance, taken pains to render accurate by repeated trials.

EXPERIMENTS

To determine the latent Heat of Zinc.

Experiments.	Latent Heat by firſt computation.	Correction for Heat received by the glaſs.	Whole latent Heat.
1	490	28	518
2	476	22	498
3	443	32	475
Mean	469		493

In theſe experiments no allowance is made for the heat loſt by ſteam, which, however, by the dexterity acquired by practice, I was enabled to render very ſmall.

Lead I found to melt at a point above 584°; that is, the mercury in my thermometer immerſed in ſemifluid lead roſe to 584°, and was continuing to aſcend, when, owing to the ſhortneſs of the tube, I was obliged to withdraw the inſtrument, to prevent its burſting. I ſuppoſe therefore that 594°, as found by Dr. Irvine, cannot be materially diſtant from the truth. Aſſuming it as true, I proceeded to make ſeveral experiments on the latent heat of lead. In doing this, I was ſpeedily led to notice, that melted lead does not by any means produce ſo much ſteam as many other metals do, when poured into water, even under the moſt careleſs management. This is the more ſurpriſing as the melting point of lead is conſiderably higher than that of ſome of theſe. The reaſon of this fact will appear from the following table:

EXPERIMENTS

To determine the latent Heat of Lead.

Experiments.	Latent Heat.	Correction.	Whole latent Heat.
1	127°.8	10°.3	138°.1
2	142°.8	11°.	153°.8
3	149°.9		
4	161°.8		
5	131°.5		
Mean	142.°7		145°.9

In theſe experiments I have by an accident, loſt the notes of the weight of the veſſel employed, in all but the two firſt inſtances. Conſequently I have been unable to add the correction for the heat abſtracted by the glaſs. Notwithſtanding which it may be fairly inferred, that the mean latent heat of lead is about 150°, a quantity certainly unexpectedly ſmall, and which, in many more experiments than thoſe related, I was at pains to examine, without being able to diſcover any material inaccuracy. In the ſecond experiment, where a little ſteam was formed, I aſcertained the quantity of water loſt to be four grains. If theſe four grains be ſuppoſed, as in the caſe of biſmuth, to contain 470° of heat, the computed addition to the latent heat of lead will be 20°.3, making in all a little more than 162°. This is certainly a very peculiar and unexpected quality of this metal.

The only other ſubſtance which I have ſubmitted to examination, for the purpoſe of aſcertaining its caloric of fluidity, is ſulphur. The melting point of ſulphur is commonly ſtated, in elementary works, to be at 212°. But that this is not accurate, any perſon may convince himſelf, by immerſing a quantity of ſulphur in boiling water, where it remains altogether unaltered. By every trial, which I have been able to make, I am convinced that the fuſion of ſulphur takes place about 226°. I ſay about, becauſe the communication of heat among the particles

of ſulphur is extremely ſlow, and the thermometer is often incruſted with ſolid ſulphur, which ſomehow or other, certainly cools below the liquid in which it is immerſed. In experiments undertaken for aſcertaining this point, the ſulphur ought to be kept in conſtant motion. The reſults of my experiments for finding the latent heat of ſulphur, are ſtated in the following table:

EXPERIMENTS

To aſcertain the latent Heat of Sulphur.

No.	Latent Heat.	Correction.	Whole latent Heat.
1	144°.56	8°.	152°.56
2	131°.	7°.	138°.
3	140°.	4°.5	140°.5
4	136°.		
Mean	137°.89	Mean	143°.68

No ſteam was formed in theſe experiments, conſequently no correction of that kind is neceſſary as in the former inſtances. I have here taken the capacity of ſulphur for heat at .189, from ſome previous trials of my own, which does not differ materially from Mr. Kirwan's determination. In the other caſes I have truſted chiefly to the numbers given by Dr. Thomſon, in his Syſtem of Chemiſtry, which I hope are correct. In every inſtance I have ſuppoſed the ſemi-liquid to have the temperature of the melting point, which I believe is generally true;

but ſome practice is required to ſeize the moment before the frozen particles float in the fluid ſubſtance. In experiments on ſulphur eſpecially, inattention to this circumſtance cauſes very great inaccuracy, and was the cauſe of conſiderable embarraſſment to me before I obſerved the ſource of my error, which I firſt was led to diſcover from the extraordinary ſmall number of degrees which the calculation gave for the latent heat in ſome of my experiments where this precaution had been neglected.

I ſhall now proceed to give a comparative view of the caloric of fluidity of all bodies hitherto examined for the purpoſe of aſcertaining that point.

TABLE

Of the latent Heat of all Substances hitherto examined.

Substance.	Melting point.	Latent heat	Latent heat in degrees meaſured by the capacity of water.
Ice	32°	155°.555	140°
Spermaceti	113°	145°	46°.4
Bees' wax	142°	175°	
Sulphur	226°	143°.68	27°.145
Tin	442°	500°	33°
Bismuth	476°	550°	23°.65
Lead	594°	162°	5°.604
Zinc	700°	493°	48°.3

In all theſe inſtances the latent heat is expreſſed in degrees meaſured by the capacity of the

relative ſolid, excepting in the caſes of ſpermaceti and of bees' wax, which are in degrees meaſured by the capacity of the fluid. I endeavoured to rectify this ſo as to make the compariſon more fair, by determining the ſpecific heat of ſolid wax and ſpermaceti; but I have not been able to ſatisfy myſelf with either of theſe points, owing to the ſoftneſs and conſequent abſorption of latent heat which a very low degree of heat induces in both theſe bodies. The numbers expreſſing their latent heats, are therefore, in all probability ſomewhat too low.

On inſpection of the above table, there does not appear any ratio by which the quantity of caloric of fluidity ſeems to be guided; it certainly does not increaſe with the difficulty of fuſion, whether we compare the number of degrees as meaſured by a common ſtandard as the capacity of water, or each by the capacity of the relative ſolid body. The moſt likely conjecture is that the quantity of the latent heat has ſome connection with the proportion of the capacity of each ſubſtance in its ſolid and fluid ſtate. If, however, this even be ſo, there is little probability of any advantage being derived from the knowledge of it. At leaſt difficulties, probably for ever inſurmountable, will prevent the determination of the ſpecific heat of any of the fluid metals excepting mercury.

I have thus endeavoured to add my mite to the information formerly poſſeſſed reſpecting the latent heat of bodies in general. The ſubſtances now exa-

mined are, thoſe which afford the readieſt and moſt certain means of aſcertaining this point. Moſt of the remaining metals melt at ſo high a temperature; that the method of trial by water would be both dangerous and inaccurate. The ſame is true of moſt other bodies, as glaſs, ſtone, &c. A few ſubſtances of the reſinous and greaſy kind might indeed have been added to the liſt: but beſides that the capacity of the latter cannot be eaſily determined in the ſolid ſtate; the former become ſo extremely viſcous, by the application of heat, that their diffuſion amongſt water would be very difficult, if not impracticable, with ſufficient rapidity. There is one of the metals, however, which fuſes at ſo low a temperature, that the embarraſſments ariſe more from the trouble of procuring a ſufficient quantity of the metal in a ſolid form, than from any other cauſe. It will be eaſily conjectured that I allude to mercury. I had, at one time, hopes of being able to add the latent heat of this body to the number above ſtated. I have been unable, however, to do this, ſolely from the want of an opportunity of freezing a proper quantity of that metal. I will now, however, deſcribe briefly the manner in which ſuch an experiment was intended to have been conducted by me, in the hope that ſome perſon, in more favourable circumſtances, may be induced to put it into effect. A conſiderable bulk of mercury ſhould be frozen; the larger this is the more probability would there be of an accurate reſult. The temperature of the apartment ought to

be as low as conveniently it can be made. The mercury ought to be encloſed in a thin glaſs veſſel.—There muſt then be provided another glaſs veſſel, with a known weight of fluid mercury: into this the ſolid metal muſt be ſuddenly plunged, and the new temperature noted as in the experiment upon biſmuth. If all or any part of the veſſel containing the ſolid mercury ſhould enter into the mixture, the effects of it, in ſubtracting its ſhare of the caloric, might be eaſily eſtimated. It would be difficult to aſcertain the preciſe temperature of the ſolid mercury, the rapid effect of heat upon it not allowing time for ſuch an obſervation. But it might be ſafely taken at —39, without danger of any error exceeding one or two degrees; and conſequently all the additional degrees of heat which it obtained, by mixture with the fluid mercury, are to be conſidered as being meaſured by the capacity of fluid mercury. Whence, after making the proper corrections for the heat given out or received by the air and veſſels, the latent heat of mercury is expreſſed by the difference between the number of degrees by which the ſolid is raiſed in temperature, and the number which an equal weight of the fluid would have been depreſſed: the latent heat, in this caſe would, as in thoſe of ſpermaceti and bees' wax, be in degrees meaſured by the capacity of the relative fluid.

In the Philoſophical Tranſactions of London, for 1804, there is an extremely ingenious and intereſting paper, by Mr. Gregory Watt, upon the regulated refrigeration of melted baſalt. That gentleman,

among various obſervations, which do not relate to our preſent ſubject, remarks, that the large maſs of baſalt, which he fuſed in a furnace where iron was commonly melted, was ſaid, by the workmen, to require only about one half of the quantity of fuel for its fuſion than an equal weight of iron would have done. Now it ſeems to me not impoſſible to draw ſome concluſions from theſe facts, which may give us a little information regarding the latent heat of iron and baſalt. In the firſt place, it muſt be ſuppoſed, that the quantities of heat produced and entering into theſe bodies, are to each other as the quantities of fuel conſumed. That is, if a weight of fuel repreſented by 2, produces an elevation of temperature in the body immerſed in it as 20°; double the weight would produce double the riſe. This is probably not exactly correct, but there ſeems reaſon to believe that it approaches to the truth: and therefore, that whatever quantity of caloric enters into iron during its fuſion, in Mr. Watt's furnace, only one-half of that enters into an equal weight of baſalt, fuſed under ſimilar circumſtances. This is not to be conſidered as applying to temperatures, which conſiſt of degrees of various value; but to the abſolute heat, according to Dr. Irvine's expreſſion. If then a quantity of caloric, as 1, has entered into the baſalt, which has been ſufficient to fuſe that ſubſtance, and to raiſe its temperature to 90° of Wedgwood, which is its melting point, and probably little or nothing higher; and a quantity of caloric, as 2, has entered into an equal weight of iron,

and proved ſufficient to fuſe it, and to raiſe its temperature to 158° of Wedgwood, at which degree it melts: how are we to proceed to diſcover what portion of caloric has in theſe inſtances ſerved to raiſe the temperature merely, and what has entered into theſe bodies as latent heat? The capacity of iron for heat is by all experiments about .12; the capacity of baſalt, as far as I know, has not been examined. Had I been able to procure a ſpecimen of the Rowley Ragg, upon which Mr. Watt's experiments were made, I ſhould have taken care to aſcertain that point. But though we are not in poſſeſſion of the preciſe number expreſſing the ſpecific heat of baſalt, we are not wholly without ſtrong analogies to guide us to a probable approximation. Baſalt is, by fuſion, converted into a glaſs. Now, if any table of the capacities of bodies be inſpected, it will be obſerved, that the capacities of the various kinds of glaſs do not take an extenſive range, but approach nearly to one another. For the ſake of illuſtration, I ſubjoin a table, affording a comparative view of the different determinations of this point:—

GLASS.	CAPACITY.	AUTHORS.
Crystal . . .	.1929	Lavoisier
Swediſh Glass .	.1923	Wilcke
Flint Glass . .	.174	Kirwan
Crown Glass .	.200	Irvine
Green Bottle Glass	.173	Author
Mean	.187	

It appears, from this ſtatement, that the capacities of glaſſes, of various component parts, do not differ very widely from one another. I ſhall, therefore, till ſome experiment is made to aſcertain the point, aſſume the ſpecific caloric of baſaltic glaſs at .18, in the confidence that I ſhall not have wandered very far from the truth. There is the leſs neceſſity of being perfectly preciſe in this number, ſince this is intended rather as an illuſtration of the mode of reaſoning, to diſcover the latent heat of bodies which melt at high temperatures, than as an actual determination in the preſent inſtance.

When baſalt is fuſed, it has received an augmentation of temperature of 90° above the zero of Wedgwood; to which muſt be added 8° more, which it has received below that point, making in all 98 degrees. Iron, during its fuſion, receives 158° + 8 = 166 degrees. Therefore, putting the latent heat out of the queſtion at preſent, the baſalt has received 98° of heat, and the iron 166°. But each degree of temperature in baſalt is worth more than each degree in iron, in the proportion of 18 : 12, or 3 to 2. Whence theſe 98° are equal to 147°, meaſured by the capacity of iron; and the baſalt has received, in augmentation of its temperature, a quantity of heat = 147; but the iron has received 166°. The difference is 19°, which are really all the heat entered into the iron more than what entered into the baſalt, excepting as far as latent heat is concerned. But we know that the heat received by theſe two bodies has been as

1 to 2; or that twice as much heat has entered the iron as has entered the baſalt: the temperatures produced, however, are only as 147 to 166, or as ſomething more than 7 to 9. Only two-ſevenths more of caloric appear to have gone to raiſe the temperature of the iron, than to raiſe that of the baſalt, although double the quantity of caloric has been expended. From which it ought to follow, that the latent heat of baſalt is what would remain after ſubtracting 147° from a quantity of caloric as one; and the latent heat of iron is what would remain after ſubtracting 166° from a quantity of caloric as two. Therefore, let the quantity of heat produced by the fuel employed to fuſe the baſalt, be expreſſed by 200°: if any other number ſuit the fancy of the reader better, he can make the ſubſtitution for himſelf, and the reſult will be the ſame. Let then that quantity of heat be called 200°: it is obvious, that the heat produced by the fuel employed to melt the iron muſt be called the double of that number, or 400°. From the 200° ſubtract the 147° of temperature received by the baſalt, and there remains 63°, which cannot be accounted for by the mere heating of the baſalt, and muſt therefore be its latent heat. In the ſame manner, from the 400°, take the 166° of temperature received by the iron, and there remains 234° for the latent heat of iron. It is true, our preſent argument does not go ſo far as to prove that theſe are the actual numbers expreſſing the latent heat of theſe ſubſtances; but it renders it probable,

that the latent heat of basalt is, to that of iron, in the proportion of 63° to 134°.

If this reasoning be admitted as correct, it might be possible to determine, by experiment, the proportions which the latent heats of various other bodies bear to each other. All that would be necessary for this purpose would be, to know the melting points of these substances, and the quantity of fuel which was necessary to convert them into fluids, together with their specific heats. These things being ascertained, we might proceed somewhat in the manner of which the above is a specimen. By operating on very large quantities of the body examined, the chances of inaccuracy would be materially diminished; and though in this way, it is clear that we never could learn the precise quantity of the latent heat of any body, but only the ratio in which one number stands to another; yet, it is not impossible, that means might be found to connect this chain of ratios with another chain, of which not only the ratios, but the quantities were known. This might be done if the latent heat of any one body, of which the ratio could be determined by the method just stated, could be found precisely, either by its combination with water, or with any other substance, such as mercury, which might be more convenient. The latent heat of the rest might then be discovered by a simple rule of arithmetic.

It is probable that the calorimeter might be applied to the determination of latent heats, in cases where

the ordinary plan could not be employed: at leaſt, it ſeems poſſible to encloſe in a veſſel, of which the ſpecific heat is known, a determined quantity of any body in a ſtate of fuſion. It is unneceſſary to deſcribe any further the method of proceeding in this caſe, which muſt be obvious to any perſon converſant on theſe topics. Perhaps the objections againſt the accuracy of the calorimeter would not be ſo forcible, in ſuch circumſtances, as they have been ſuppoſed to be in many other caſes.

ESSAY II.

ON THE AFFECTIONS OF SULPHUR WITH CALORIC.

Sulphur has been an object of attention to the chemiſts from a very early period. They have examined it in various ways, and ſubjected it to the ordeal of innumerable experiments, by means of which they have ſucceeded in making many diſcoveries regarding its qualities, not leſs uſeful in the ſcience of chemiſtry, than in the practice of the arts. In one reſpect, however, leſs progreſs has been made than might have been expected. The affections of ſulphur, by caloric, have not been ſo much attended to, nor have experiments on the operation of heat upon it been ſo numerous or frequent as the importance and familiarity of the object ſeem to have demanded. If we except Mr. Kirwan's determination of the ſpecific heat of this body, ſcarcely any

other obſervation has been made at once with the merit of accuracy and utility. I do not mean to ſay that no obſervations at all have been made, or that thoſe have been wholly deſtitute of claims to praiſe; but that leſs has been done in this way than on many other ſubjects, and that room is ſtill left for additional remarks.

Sulphur, at the ordinary temperature of the atmoſphere, is always a ſolid, brittle, hard body, which expands rapidly and unequally when heated, owing to its being an extremely indifferent conductor of caloric; ſo that the external parts expand more quickly than the internal, and are thus forced to a ſeparation. Cold, as far as is known, produces no remarkable effect upon this body. When ſubjected to friction it accumulates on its ſurface a large quantity of electric matter; conſequently it is a non-conductor of that fluid, ſince it is impoſſible to collect electricity on a body that freely diſperſes it in every direction. The differences between ſolid and fluid ſulphur in their relations with the electric fluid, are not well underſtood. A ſpark can certainly, as I have found, be drawn through fluid ſulphur, which is then thrown into violent agitation: but it may be doubted whether it paſſes through the ſubſtance of the ſulphur or merely forces the particles out of the way: the ſubject is worthy of further inveſtigation. The non-conducting power of ice, contraſted with the oppoſite quality of water, is extremely remarkable in a chemical point of view.

The ſpecific heat of ſulphur, according to my experiments, is .189, which does not differ much from Mr. Kirwan's determination, and may be conſidered as confirming the accuracy of his obſervation. The melting point of this body I have found rather higher than it is uſually eſtimated, and to be at 226° nearly. In making this experiment, the thermometer is apt to be encruſted with ſolid ſulphur, in conſequence of which the mercury falls lower than it ought to do, but that may be prevented ſimply by keeping the thermometer in conſtant motion.

Sulphur, in all probability, changes its capacity for heat ſuddenly during fuſion: at leaſt it remains hard nearly to the very laſt, and flows into an extremely thin liquid. It may be frozen and melted repeatedly, without any alteration in its properties. If the application of heat be continued for ſome time, a very ſingular change takes place in the appearance of the ſulphur: inſtead, like other bodies, of continuing to become thinner and thinner by the increaſe of temperature, and at laſt being converted into an aëriform fluid, this ſubſtance grows very thick and tenacious; and though its ſolidity cannot be reſtored in this manner, the coheſion between its particles is very greatly augmented. It is aſſerted that no chemical alteration of the body is effected by this proceſs: no oxygen, or very little, is ſaid to have been abſorbed; and the ſulphur, after undergoing the thickening, produces nearly the ſame quantity of ſulphuric acid as before. Its colour,

however, is deepened by fuſion; and though, at firſt, ſome of the colour vaniſhes after cooling, yet repeated applications of heat, in this manner, permanently darken the hue. This thickening, as I find, can be repeated any number of times upon the ſame maſs of ſulphur, and is regularly followed by the diminution of the thickneſs on the removal of the high temperature. The ſhape of the veſſel, as far as I have obſerved, does not influence the appearances, provided enough of heat be applied. The unreſtrained or the difficult acceſs of air does not ſeem to be more concerned. The whole clearly depends on the agency of caloric; and if the proper temperature be afforded, the deſired effect never fails to appear. By many experiments I have obſerved, that this thickening begins at 320° of Fahrenheit's thermometer. The thickneſs continues, and even increaſes to the temperature of 530°, and even 550°; at leaſt, nothing like a greater fluidity can be noticed, although conſiderable fumes ariſe. It cannot then be doubted, that this thickening of ſulphur is purely effected by heat; that above the temperature of 320° it cannot be prevented, and may be removed and renewed any number of times, in the ſame maſs, by a proper attention to the temperature of the body.

This is very unlike the uſual effect of caloric upon bodies, which, in moſt other inſtances, uniformly tends to augment the fluidity of ſubſtances ſubmitted to its action, excepting in a few, which, during their paſſage from the liquid to the ſolid ſtate, ſuffer

an expanſion. Water alſo, from 32°, Fahrenheit, to 40°, has been aſſerted to contract as its temperature riſes. There has been, eſpecially of late, ſome diſpute upon this ſubject, which has become of more importance ſince it has interfered in the evidence of Count Rumford's ingenious opinion regarding the tranſmiſſion of heat through fluids. It ſeems probable that we ſhould have ſome chance of deciding the fact of the expanſion or contraction of water immediately after fuſion, by examining its ſpecific gravity. I propoſe to try this as ſoon as I ſhall have a proper opportunity and apparatus. In this manner, at leaſt, very ſmall variations of denſity may be detected.

It became an object of ſome curioſity, to learn whether this thickening of ſulphur is accompanied by an expanſion or contraction of its volume. I examined this point by filling an ounce phial completely to the very brim with melted ſulphur: I then applied heat till the temperature was about 400°, and the ſulphur very thick and tenacious, and emitting a little vapour. The proceſs of cooling was then carefully obſerved, and it was remarked that a ſteady contraction of the fluid enſued till it arrived at the temperature of 226°, at which point it began to expand, and thruſt upwards a large nipple-like projection. The ſum of the contraction, from 400° to 226°, amounted, as I computed, to an eighteenth part of the volume of the ſulphur at 400°, and to a ſeventeenth part of its volume when fluid at 226°.—

The expanſion, during freezing, may leſs accurately be taken at one-fortieth of the maſs of fluid ſulphur. Theſe things being admitted, it ought to follow that a piece of ſolid ſulphur, at 226°, ſhould ſwim in fluid ſulphur at the ſame point: but this does not happen if the temperature of the ſolid is conſiderably below the point of fuſion. This may be accounted for by ſuppoſing, what is otherwiſe extremely probable, that ſolid ſulphur expands rapidly by heat, and conſequently contracts equally rapidly by cold: ſo that at a certain number of degrees below 226°, its ſpecific gravity becomes equal to that of its correſponding fluid, and at every degree of heat lower, than that it muſt inevitably ſink till its temperature is duly raiſed. Some pieces of cold ſulphur, which I threw into the ſame ſubſtance, melted, ſunk gradually, and were almoſt immediately fuſed, that is to ſay, the fuſion took place at the ſurface before the ſulphur, which is a very bad conductor of heat, could receive enough to be expanded, ſo as to riſe in the fluid.

It was formerly obſerved by my father, that melted ſpermaceti and wax cooled in ſuch a manner as to render it probable that they parted with their latent heat gradually. I thought it worth while to try in the caſe of melted and thickened ſulphur, whether any thing could be remarked that ſhould indicate a change of the capacity of that body for heat. With this view I made ſeveral experiments, of which here follow the reſults.

EXPERIMENTS

On the Cooling of melted Sulphur.

Time.	Experim. I. Temp.	Diff.	II. T.	Diff.	III. T.	Diff.	IV. T.	Diff.
Min. 1	530	0	550	0	496	0	470	0
2	511	19	530	20	84	12	62	8
3	484	27	507	23	73	11	48	14
4	457	27	486	21	60	13	34	14
5	436	21	466	20	47	13	21	13
6	416	20	448	18	35	12	408	13
7	398	18	432	16	22	13	397	11
8	386	12	417	15	10	12	83	14
9	374	12	403	14	398	12	71	12
10	364	10	390	13	386	12	360	11
11	357	7	376	14	374	12	346	14
12	350	7	364	12	362	12	330	16
13	343	7	350	14	351	11	314	16
14	333	10	336	14	340	11	302	12
15	323	10	318	18	328	12	291	11
16	312	11	303	15	315	13	284	7
17	303	9	293	10	305	10	274	10
18	294	9	284	9	296	9	269	5
19	285	9	276	8	286	10	262+	7
20	278	7	269	7	275	11	256	6
21	271	7	264	5	267	8	249	7
22	264	7	257	7	262	5	243	6
23	257	7	250	7	57	5	237	6
24	251	6	244	6	50	7	231	6
25	245	6	238	6	45	5	226	5 freezing
26	239	6	232	6	39	6		
27			226	6	33	6		
28					27	6		
29					226	1		

The quantity of ſulphur employed in theſe experiments varied conſiderably in the different inſtances from two ounces to ſix. There evidently appear conſiderable irregularities in the progreſs of cooling, which muſt be attributed to currents of air impoſſible to be prevented at ſuch high temperatures. In ſuch caſes it is very difficult to draw unobjectionable concluſions. But from an inſpection of the above table, it appears that ſome retardation in the progreſs of refrigeration took place about the temperature of 350°, a little above, and a little below, from which it ought to follow, that near that point ſome enlargement of the ſpecific heat muſt have occurred, ſince no other cauſe would account for the retardation at the ſame point, in all the experiments related.

The thickening of ſulphur by the application of caloric, is curious in another point of view, when we conſider the proceſs by which bodies are converted from the ſolid to the fluid ſtate, and from that to the vaporous, as well the general expanding, and liquefying property of this principle. It appears pretty evidently that a ſolid body differs chiefly from its correſponding fluid in the difficulty or impoſſibility of moving its particles upon each other. The particles of the fluid, on the contrary, move over each other with the utmoſt freedom. Now to convert a ſolid as ice or ſulphur into the liquid form, it might ſeem likely, at firſt ſight, that the

caloric, entering the ſolid, by its expanſive and repelling powers, deſtroyed the coheſion between the particles, and permitted them to ſlide ſmoothly and eaſily over each other, or, in other words, made a fluid of it. But though this explanation does not lie open to many objections in the caſe of thoſe bodies which expand during fuſion, that is not the caſe where a contraction takes place during that proceſs, as happens in the inſtances of ice and ſulphur. It is clear that caloric cannot produce liquefaction in theſe ſubſtances ſolely by deſtroying coheſion, ſince, as far as we know, coheſion increaſes as the diſtances diminiſh, in a ratio not aſcertainable. Therefore no contraction of any body can poſſibly cauſe its particles to have leſs attraction for each other. Of conſequence, ice, which contracts during fuſion, could never be melted by heat, if heat produced no other effect on it than to diminiſh the coheſion of its particles.

In order to ſolve this difficulty, recourſe has been had to another principle, better adapted for the purpoſe. It has been ſaid that the particles of ice have a certain degree of polarity, a diſpoſition to aſſume a peculiar ſituation, in fact that they have a ſpecific power of adhering to each other by their ends or otherwiſe, and this in alluſion to the well-known and ſtriking facts of magnetiſm, has been called their polarity. It cannot, however, be like the magnetic polarity, as far as we know, in all reſpects. It is not certain,

nor even probable, that theſe particles arrange themſelves in any way relative to the poſition of the poles of the earth. All that is meant to be conveyed by this expreſſion is, that the particles of ſolid bodies do not ſimply adhere, but adhere with a much greater force at one point than at any other. Caloric, therefore, by deſtroying this polarity, may effect a compreſſion of the bulk of a fuſing body, the particles no longer refuſing to occupy the interſtitial ſpaces. All ſolid ſubſtances however, do not undergo the ſame proceſs when expoſed to the action of heat. Many of theſe, it is well known, do not contract when melted, and, conſequently, either have no polarity of particles to be deſtroyed, or their polarity is ſuch that the particles adhere together preciſely in the manner in which they occupy the leaſt room. We are very far from underſtanding what is the nature of this polarity, whether all bodies poſſeſs it, what relation it has to the general properties of matter, or what to the peculiar phenomena of magnetiſm. Theſe inquiries are, however, not only very curious in their own nature, but are likely to throw conſiderable light on the operations of chemiſtry when they ſhall receive a ſatisfactory inveſtigation.

The phenomena of melted ſulphur which led to theſe obſervations are alſo to be conſidered as ſhewing the exiſtence of this quality, which is named polarity, in a ſituation where it has not before been remarked. If we are right in concluding the thick-

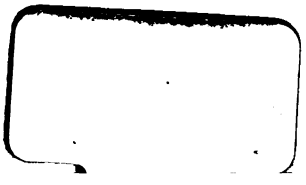
ening of ſulphur to be purely an effect of caloric, and an effect unaccompanied by any contraction of bulk, but, on the contrary, by an expanſion, it muſt be ranked in the ſame place with the freezing of this body which, as we have ſhewn, is alſo accompanied by an expanſion, and a polarity of particles. Now ſurely it is a very ſurpriſing thing that at the melting point, 226°, caloric ſhould have the power of deſtroying the polarity which cauſes the ſolidity of ſulphur, but that the addition of ninety-four degrees of caloric, ſo far from confirming the fluidity, ſhould actually appear to reſtore the polarity of the particles of this body, at leaſt in a certain degree. It appears very difficult to reconcile theſe phenomena with each other. The caloric acts apparently in two manners directly contrary, and if we allow the polarity of ſolidity to be deſtroyed by heat, it will be hard to explain how the polarity of incraſſation ſhould ariſe from the ſame cauſe. I do not pretend, at preſent, to be able to give a ſatisfactory elucidation of this difficulty, which ſeems to me to be very little conſiſtent with ſome parts of the moſt generally received theories of caloric. The moſt natural inference that occurs to me is, either that both ſorts of polarity have their origin from another cauſe than caloric, or at leaſt that in one of the inſtances another agent may be ſuſpected. We are expoſed to many difficulties in arguments of ſo complex a kind as the preſent, and conjectures ought not to be admitted without very ſolid grounds. But it is ſcarcely poſ-

ſible to imagine the interference of any other influence in the deſtruction or ſuſpenſion of polarity, excepting that of the electrical matter. But beſides this, there are at any rate, conſiderable reaſons for believing that electricity actually exiſts as a component part of many and probably of all bodies. The quantities of it in different bodies cannot be doubted to vary, and ſince we well know that the conducting powers of ſubſtances differ from each other, ſo in all likelihood do their capacities or ſpecific electricities. The phenomena of galvaniſm ſupport the ſame opinions. If then bodies contain the electric fluid as one of their component parts, it is not to be ſuppoſed that they will paſs from the ſolid to the fluid ſtate without ſome effect being produced upon their electricity. In the caſe of ice, we know that ice is a non-conductor, but that water is a tolerable conductor. Sulphur alſo is a non-conductor, and liquid ſulphur conducts ill; it does not, however, wholly refuſe a paſſage to the electric ſpark, though the difficulty is conſiderable. Ice therefore, it is not improbable, receives during its fuſion a quantity of electricity, which may deſtroy the polarity of its particles and contribute to the formation of water, and in the ſame manner ſulphur during its fuſion. If this were a juſt account of the matter, either of theſe bodies being raiſed to a high temperature, and having at the ſame time their capacity for electricity diminiſhed, would immediately become thicker, the antipolar influence being in part removed; this we

ſee happens in the inſtance of ſulphur. I do not mean to offer theſe remarks as any more than conjectures of which the truth is very uncertain, and very difficulty referable to experiment.

The capacities of bodies for electricity cannot be exactly found, for want of an accurate electrometer, which ſhould be as good a meaſure of electricity as the thermometer is of heat. But ſomething might be done in this way; by having a ſtandard Leyden phial charged, ſo as to produce a certain effect on the electrometer, and diſcharging it into another phial filled with the body of which the capacity is required, ſome idea might be obtained of the quantity of electricity neceſſary to abſtract from the charged phial to eſtabliſh an equilibrium with the other. And this quantity, which would, probably vary according to the body examined, would, if equal weights or bulks were uſed, give at leaſt the order in which ſubſtances ſhould be arranged; many precautions would of courſe be required to obtain even a ſmall degree of acuracy of reſult in ſuch experiments. One obſervation, however, has been made upon metals fuſed by electricity. They do not undergo real fuſion, but the polarity of their particles is deſtroyed without the neceſſary quantity of heat being preſent to retain them in fuſion, and the whole is converted into a powdery maſs, where probably ſome particles are diſengaged and ſome imperfectly joined to thoſe neareſt to them. I was unable to fuſe ice in this manner, but a piece of that body

equal in this reſpect to the roſin, and the ſulphur eſpecially conſiderably ſuperior. Whether the account here ſuggeſted of the method in which fuſion is effected has any foundation or not is certainly very doubtful. I am far from attributing to it any conſiderable degree of probability, and am ſenſible that it can as yet only claim admiſſion into the liſt of conjectures. Arguments on topics of ſo abſtruſe a nature are always conducted with difficulty, and never without danger of error. But however this point may be determined, I can revert with more confidence to my original propoſition, that in the proceſſes of the fuſion and incruſtation of ſulphur ſome other agent than caloric is moſt probably engaged. And this is equally true, though we may be unable to detect the preſence or explain the properties of that agent, though our ſpeculations upon the interference of the electrical fluid may prove deſtitute of foundation, and though we may be compelled to imagine ſome undiſcovered property of matter to act this part, to perform which caloric alone is not adequate.

FINIS.

W. Flint, Printer,
Green Arbour court, St. Sepulchre's.